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AN IMPROVED DYNAMOMETER.

By WILLIAM P. TATHAM.

The mechanical world has long desired a correct and convenient dynamometer, capable of general application. All of the instruments heretofore presented fail in some of the desired qualities. The Proney Brake, which is the most correct, absorbs the power, and does not transmit it. Dynamometers of the Morin type, which are next in accuracy, are of limited application to small powers, and of the many others, it would be hard to find one in which the friction of the instrument in not largely represented in its indications.

To the old requirements for a good dynamometer we have now superadded the demand growing out of the generation of electricity from mechanical power, and accuracy is here, as ever, the most to be desired. Under these circumstances, which sufficiently attest the difficulties of the case, I venture to describe an instrument involving the application of a new principle, in the hope that it may satisfy a pressing want.

Referring to Fig. 1, the arrows show the movement of the belts; the frame of the machine is omitted, the more clearly to exhibit the working parts. Let A represent the first motion pulley of the dynamometer, upon a shaft receiving power from any source outside; B B′

two semicircular vibrating frames, having freedom to move around their central knife edges, C, C′, which play in valleys formed by the intersection of two small planes. These frames are linked together by two links at D and D′, also on knife edges. The frame B carries two pulleys, E and F. The pulley E is adjusted and centred upon the frame so that the central knife edge C coincides with the point of tangency of the belt (a) with the pulley E, the tangent point being taken at the middle of the thickness of the belt. By this arrangement the force of the belt passes directly through the fulcrum of the frame B, and therefore exerts no influence to vibrate it.

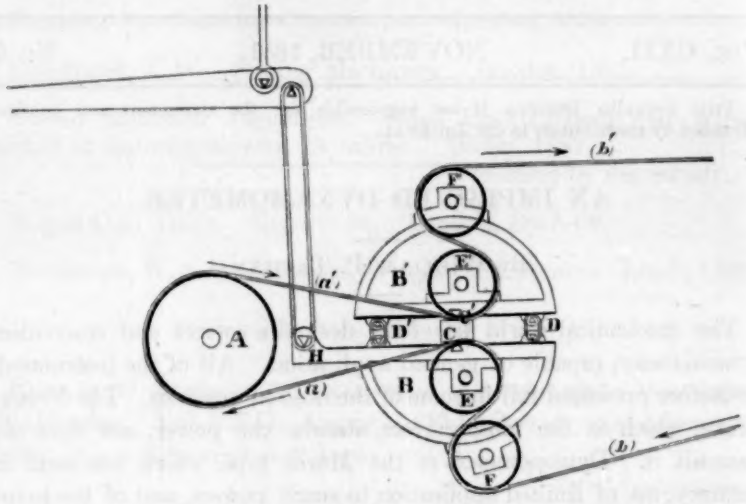


Fig. 1.

The pulley F is adjustable around the circumference of the frame B, and is placed in such a position that the belt (b), passing from the machine on trial, makes at its point of tangency with the pulley F a right angle with a straight line joining the tangent point to the central knife edge C. The frame B has an arm attached to it, to carry the knife edge H, to which the link of the scale beam is attached. The distance of this knife edge H from knife edge C is equal to the distance of the latter from the point of tangency of pulley F and belt (b), taken as before at the middle of the thickness of the belt.

The result of this disposition is that the only influence to cause a vibration of the frame B is the reaction of the belt (b), which is exactly equal to the action of the same belt upon the machine on trial. In

the same manner the action of the slack belt (b′) upon the frame B′ is exactly equal to its reaction upon the machine on trial. The frames B and B′ being connected together by the links and knife edges, the difference of the tensions on (b) and (b′) is exerted to vibrate the two frames, and this difference alone is felt by the scale beam, all friction being eliminated except the friction of the knife edges. The rudely constructed working model which has been tried indicates with surprising accuracy when tested by a brake. To prove the elimination of the friction, the wheels E′ and F′ where chocked without change of indicated power.

I see no reason to doubt that this machine, when properly conducted and adjusted, would indicate correctly the power absorbed by the train of a watch, or given out by a water wheel; but it is still lacking in one very essential quality of a good machine—*it would not work well when out of order.*

When the journals of the wheels on the vibrating frames become worn by long use these wheels will be displaced, and their points of tangency with the belts will be displaced also, and the accuracy of the indications will be impaired.

In order to meet this requirement I have embodied my ideas in a different form, represented in Fig. 2. The arrows, as before, show the direction of the belts. A is the first motion pulley and shaft; a and a′ the tight and slack belts to the pulleys carried on the vibrating frame B. These belts do not pass through the point of tangency, but *their direction does.* The vibrating frame B is balanced upon the knife edges C, and is provided with knife edges H, which engage the links of the scale beam.

The distance from C to H is equal to the effective diameter of the pulleys E, E′ upon the vibrating frame; b and b′ are the tight and slack belts from these pulleys to the pulley M, which now takes the place of the machine on trial in the description of the machine represented in Fig. 1. It is important that the belts b b′ should not make a less angle with the vertical line than the belts a a′.

The index will show the force exerted on the pulley M, whose friction will be included. The friction of the pulley and shaft M can be estimated accurately by providing simple means for weighing the combined tensions of the belts b and b′ upon the pulley M, and then, with the indications furnished by the dynamometer when running light and loaded, the friction when loaded can be calculated.

The speed of the belt may be measured by the counter, and the whole indication of power may be ascertained by combining a multiplied movement of the vibrating frame with a regular movement of a paper band fed from one of the shafts.

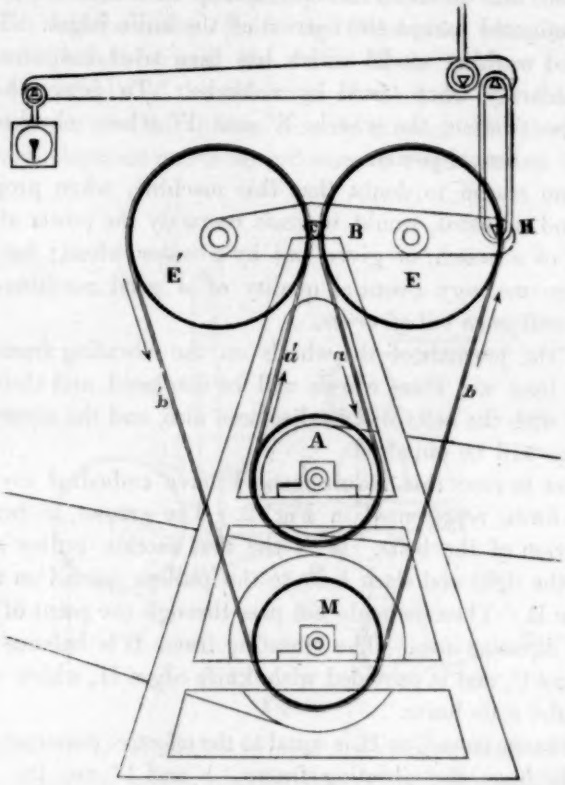


Fig. 2.

A critical examination of the last-described machine will show that no probable wear and no probable change of the thickness of the belt can interfere materially with the accuracy of its indications.

The vibration of the frames need be very small. In the working model (if it were strong enough to bear it) 320 lbs. would be indicated by the spring balance on the scale beam, and would correspond to a movement of ·0375 inch of the knife edge H.

In estimating the middle of the thickness of the belts as the points from which to measure the effective diameters of the pulleys governing

the feed of belt and transmission of force I have followed the books which have been confirmed by my preliminary experiments. In constructing an exact machine this point should be carefully examined.

A third form of the dynamometer, combining the accuracy of No. 1 with the wearing properties of No. 2, may be constructed by dividing the vibrating frame B of No. 2 into two frames, connected together by links, each frame having its own fulcrum C in the line of the middle of the thickness of the belts a a′, all the belts to be made vertical and pull at right angles to the levers.

CHEMICAL METHODS FOR ANALYZING RAIL-STEEL.

By Magnus Troilius, Chemist to C. P. Sandberg, London, Eng.

Read before tha American Institute of Mining Engineers, October, 1881.

Introduction.—By C. P. Sandberg.

Since the discussion on steel rails in America has forcibly drawn attention to the value of chemical analysis, if not as a necessary stipulation, at least as a guide to control the usual mechanical tests, some doubt has been thrown upon the accuracy of the analytical results obtained by different chemists.

To any one having the least acquaintance with chemistry it is quite clear that if exactly similar results are to be obtained from the same borings of steel, exactly the same methods must be used by the different analysts. Hence the necessity (if complications are to be avoided) of establishing what I may call standard or normal methods, to be used both by the inspectors and by the chemists at the works. Remembering that the application of chemistry to steel rail inspection is yet in its infancy, it is of great importance to possess a perfect acquaintance with the best methods in use.

Being myself a grateful pupil of Professor Eggertz, of the School of Mines in Sweden, it occurred to me, two years ago, that I could not do better than start a laboratory of my own, and engaged one of his pupils, Mr. Troilius, for the purpose of analyzing the steel borings from mechanically-tested rails, so that I might thus obtain, without delay, thoroughly accurate determinations.

Moreover, in order to carry on the operations in perfect accordance with the methods used at the steel-works in England and Germany, where I had to control the manufacture, I deemed it desirable to allow

Mr. Troilius to go through a course of training at these works; and I gladly seize this opportunity of expressing my grateful acknowledgments to several works in England and in Germany for affording every facility for such an exchange of information as was found necessary to arrive at the best analytical methods to be used.

The value of this preliminary training has already become evident, for, after working a year in my own laboratory, we find that the results hardly ever differ from those obtained at the works; or, at any rate, they rarely differ from the results of those professional chemists who check the same borings in case of any discrepancy.

Inasmuch as this work is principally executed for America, it naturally follows that if it is checked by American chemists it will be of interest, both for them and for the chemists on this side of the Atlantic, to know the methods of analysis followed in the two countries. With this view, I beg to introduce the following paper, which has been very carefully worked out by Mr. M. Troilius, and therefore deserves the attention of the members of the Institute.

I can only say of this paper, as I have said in my own, "On the Specification and Inspection of Steel Rails in Europe," that it fully explains the methods, which I have hitherto adopted, with excellent results. But if any better methods can be suggested by American chemists, I shall only be too glad to modify my present mode of working.

It only remains for me to add that Mr. Troilius will have great pleasure in answering any remarks or questions that may be addressed to him with reference to the following methods for the application of chemistry to steel rail inspection.

19 Great George Street, Westminster, London, August, 1881.

Chemical Methods for Analyzing Rail Steel.

Useful Appliances.—One of the most useful and necessary appliances in a steel laboratory is the hot-plate. An iron plate, 12″ x 18″ x $\frac{5}{8}$″ thick, heated from below by a good Bunsen burner, will answer very well; or, if more convenient, the plate may be combined with a coke fire and a muffle-furnace, the coke fire thus heating both the plate and the muffle-furnace. In any case, the plate should be heated in such a manner as to have a boiling temperature at only one part, from which part the heat should gradually decrease towards the edges. Thus arranged, the plate forms a very satisfactory sub-

stitute for water-baths, drying-boxes, etc., as will be seen in the following description of the methods. There is no risk of destroying beakers, etc., after sufficient experience in working the plate has been acquired.

A plate of this kind has been in use in the London School of Mines for about fifteen years, but, as far as I am informed, it was Mr. Snelus who first employed a plate in this way at Dowlais, and it has since become universally used in English and Welsh laboratories. I should also mention that it has been successfully introduced in the Stockholm School of Mines, in accordance with my suggestion last year in the *Jernkontorets Annaler*. In German steel laboratories the water-bath and other more "scientific" appliances are more generally in use than in England and Wales; in fact, I have not so far seen the plate used in any of the German steel works which I have visited. It is necessary to have the plate placed under a good draught, so as to remove all the noxious fumes which are evolved during the operations conducted on the plate.

As regards other useful appliances belonging to a well-fitted steel laboratory, they are all more or less common or are only occasionally used, and do not deserve as much attention as the above-named plate. Fluted funnels, however, are worthy of being mentioned as being somewhat quicker to work with than ordinary plain ones. I have not found them in use on the continent as frequently as in the United Kingdom, but they are now beginning to be somewhat more appreciated, even in the continental countries.

Carbon Determination.—Eggertz's color-test is a very accurate method for determining carbon in rail-steel, provided the operator has sufficient experience and takes all the necessary precautions. At the same time, this method has the great advantage of being very rapid. It is now nearly twenty years since this method was described in Sweden and Germany by Professor Eggertz, and very shortly afterwards Mr. C. P. Sandberg published an English translation of the method in the *Chemical News*. In Great Britain the color-test is now very largely used, every blow in the Bessemer converter being thus tested for carbon, and in the hands of skilled manipulators it gives every satisfaction. In German steel laboratories the color-test is not so much used as in Great Britain, and costly arrangements are often employed for carrying out determinations of carbon by the combustion process on a large scale. For the daily control of rail-steel, how-

ever, this is rather an impracticable arrangement, when results, accurate within 0·01 per cent., can so rapidly be obtained by the color-test.

'Experience soon taught the manipulators at steel-works to modify the method in many respects so as to attain greater rapidity, thus deviating from the directions given by Professor Eggertz in 1862. There has, however, been some uncertainty as to many of the details, and as some rather serious discrepancies have occasionally occurred, especially in analyzing the harder classes of steel, there have not been wanting people denouncing the whole method. It is, therefore, with great pleasure that I am able to accompany this paper with a translation of Professor Eggertz's recent article on the subject. As the contents of this article, or at least the more important points, were kindly communicated to me some weeks before it was published in Sweden, I have had ample opportunity of applying the experience thus gained, and have found it thoroughly corroborated by my own results.

The most important facts in Professor Eggertz's paper are contained in the rules given for (1) quantity of acid required for each 0·1 gramme of steels of different percentages of carbon, and (2) minimum addition of water required for each 0·1 gramme of steel dissolved in nitric acid to remove the iron color.

Referring to the complete* translation, for further information I will now explain how I carry out my determinations of carbon in rail-steel by means of the color-test. My mode of manipulation is the same as that used at most English and Welsh steel-works, with the modifications of the two above-mentioned new rules of Professor Eggertz.

I use 0·2 gramme of the steel for testing, and along with every set of samples 0·2 gramme of standard steel is dissolved. This is indispensable with the mode of procedure I adopt, no precautions being taken to exclude the sunlight, etc. The solution is effected in test-tubes 6 inches long and about $\frac{5}{8}$ inch internal diameter. The dimensions of the test-tubes are not a matter of great inportance, but they should not be too narrow.

The nitric acid I always allow to flow into the tubes from a graduated burette, this being by far the best way of adding the acid. The tubes are then put into a beaker 4 to 5 inches high, half filled with

*The accompanying translation contains an addition made after publishing the article in Sweden, and is thus believed to be more complete than any translation which has already appeared in print. See Appendix.

water. The beaker may be advantageously covered with a perforated tin plate, and the tubes put through the holes, and thus steadied. Heat is then applied, and boiling is continued until the steels are dissolved; this seldom requires more than half an hour, and is greatly promoted by the jumping of the tubes in the boiling water. When the solution is completed the tubes are put into cold water, and the determination of carbon is thereupon carried out by means of the carbon-tubes. The carbon-tubes are generally bought in sets of three tubes, one of which is graduated and the other two not. The tubes in each set are selected carefully so as to be of the greatest possible uniformity as to dimensions and quality of glass; their capacity is 20 cc.

In my ordinary work I put the standard-steel solution into one of the ungraduated tubes, measuring off by aid of the graduated tube, and in very particular analyses I use standard-steel solutions of different colors in both the ungraduated tubes. This helps the eye to catch faint differences in tint. The solutions for testing are put into the graduated tube.

The differences in the results, which are sometimes obtained when analyzing the harder classes of steel by the color-test, are not observed as far as rails are concerned, and I find no difficulty in obtaining accurate results in this case. In fact, as far as my experience goes, the carbon in rails is that element which can be most easily accurately determined, and this by the simple color-test. But even for harder steels a very much greater certainty is now secured by the method described in Professor Eggertz's latest publication.

It is always desirable, if not necessary, that the standard-steel should have a percentage of carbon not differing too widely from the average percentage of carbon in the steels for testing; especially when dealing with *very* soft steels one finds the necessity of having a soft standard.

In working this method for carbon estimation, when the carbon ranges from 0·10 to 0·80 per cent., I have obtained accurate determinations with great rapidity; and this, indeed, is the great value of the method, which is best seen by its application for ascertaining the carbon in every blow, even at the largest steel-works in England, where hundreds of charges are made per day. Ordinarily, a boy is trained to do this work, under superintendence of the chief chemist, and consequently the cost of execution is but very small.

The plan of dissolving rapidly, and then cooling the tubes, as just described, was originally employed by Mr. Snelus at Dowlais.

Phosphorus Determination.—The greater number of steel-works which I have visited use the Eggertz's molybdic method for determination of phosphorus in steel. Except in Sweden, however, it is only at one large works in Germany that I have seen this method carried out in the way originally described by Professor Eggertz, and at those works superior appliances, etc., enable the manipulators to obtain pretty quick, and certainly very accurate, results, even when using weighed filters, and working upon the small prescribed quantity of 1 gramme. At other German works it was the practice to redissolve the phospho-molybdate obtained, and to finish the analysis by the magnesia method. But at all the English and Welsh works with which I am acquainted the phosphorus is estimated by weighing the phospho-molybdate itself. Several grammes of the steel are always used, and the precipitate is generally brushed off from the filter. Only at one Welsh works have I seen it gently burnt, so as merely to incinerate the filter.

The former of the two last-named modes of manipulation is the one I use. Like so many other useful modifications in the chemistry of iron and steel, this plan was originally introduced at Dowlais by Mr. Snelus. In the following pages I will describe the process, and, at the same time, refer briefly to the Welsh "burning" method and to the magnesia method, etc.

The Burning Method.—The solution of the steel for the determination of phosphorus is an easy operation. Not less than 5 grammes of steel are dissolved in a mixture of equal volumes of strong nitric and hydrochloric acids. (I use for this purpose nitric acid 1·42, and hydrochloric acid 1·195 sp. gr.) No loss through escape of phosphorus in combination with hydrogen is hereby incurred. The solution is evaporated to dryness, and heated until all dark fumes have ceased to escape. A beaker or a porcelain dish may be used, according to circumstances, and evaporations, etc., are performed on the hot plate. By the evaporation to dryness the complete solution of the steel is secured, all organic matter is destroyed and the silica can be separated, which is advisable if it is present in any noticeable quantity.

The dry mass is then dissolved in strong hydrochloric acid, the excess of acid removed by evaporation, hot water added and the silica filtered off. (If little or no silica is present it is, of course, unnecessary to filter it off, and the precipitation of phosphorus may then at once be proceeded with.) The filtrate is evaporated down to a small bulk, so that it is only just fluid; it is allowed to cool, and then about 4 cc.

of the strong nitric acid are added. A little rinsing water is introduced so as to make the bulk about 20 cc. The beaker is strongly shaken in the right hand, while from a pipette, which is held in the left hand, 20 cc. of the solution of molybdate of ammonia are allowed to run into the beaker in a thin stream.

The solution of molybdate is prepared by dissolving 100 grams of molybdate of ammonia in 1000 cc. of water and 100 cc. of ammonia, 0·88 sp. gr. It is of no advantage to use less strong solutions of molybdate than this, as one has then to employ a larger quantity of the same, and thus obtain a greater bulk, the work being thereby retarded.

After pouring in the solution of the molybdate a few drops of ammonia (0·88) are added, and the beaker is shaken until the precipitate of iron has disappeared. The phospho-molybdate is then completely down, and you have only to leave the beaker on the less hot part of the plate at least for 1 hour, during that time allowing it to settle, and shaking it up again repeatedly. After the last shaking the precipitate must separate distinctly, and leave a perfectly clear, supernatant solution. There is no danger of getting molybdic acid down, even if you were to boil for a moment or to use a large excess of molybdate, provided that there is a sufficiently large quantity of nitric acid present; but, if there is arsenic in the steel, this will come down along with the phosphorus, and cause too high results. The using of the ammoniacal solution of molybdate and ammonia causes a considerable elevation of temperature; hence, as will be shown below, the precipitation of arsenic.

After settling, pour the liquid on a good Swedish 4-inch filter; wash the filter with cold water containing 1 per cent. of nitric acid until it is quite white; wash the precipitate in the beaker once by decanting with ordinary water, moderately hot, and finally wash the precipitate down on the filter, and collect it at the centre with as few washings as possible with ordinary water, moderately hot. The filter should be quite white before the precipitate is washed on to it.

If the washing is conducted in this way no loss will be incurred in dissolving, neither will the fluid run through turbid. The solubility of the phospho-molybdate precipitate, at 16°C., is given by Professor Eggertz as follows:

In pure water,	.	.	1 part in 10,000
In water, with 1 per cent. of nitric acid,		1	" 6,600
In hydrochloric acid, 1·12,	.	1	" 550
In nitric acid, 1·2,	.	.	1 " 190

If to the solvent be added a solution of molybdate of ammonia, equal to about half of its volume, the solvent action seems to be considerably lessened.

After washing, unfold the filter containing the precipitate upon another filter, and put it upon the edge of the plate to dry. The unfolded filter should be covered with a large watch-glass, so as to prevent dust from getting into it. As to the temperature for drying, this is by no means so essential a point as is often supposed, and the precipitate may be dried for hours at a temperature between 100° and 140°C. without changing its percentage of phosphorus in any noteworthy degree, as stated by Professor Eggertz, whose results in this respect are compiled in the following table:

Temperature.	Loss per cent. of weight of precipitate.
95°—100°C.	0·40
100°—120°C.	0·20
120°—140°C.	0·05
95°—140°C.	0·65

The total loss in weight is thus only 0·65 per cent. when drying at 140°C., and this has no practical influence, considering the small amount of phosphorus in the precipitate and the large quantity of steel operated upon. The precipitate also retains its yellow color at the temperature of melting lead (355°C.), but gets black at the temperature of melting zinc (400°C.)

When dry, the precipitate is shaken down into a weighed platinum, or porcelain dish, the brush not being applied until nothing more can be loosened from the filter by mere shaking. It is a convenient practice to hold the filter in the left hand, and to knock gently on this hand with the other.

Having thus given the outlines of my mode of using the molybdic method, I would add the following precautions, which are necessary for attaining accurate results:

1st. Removing excess of hydrochloric acid from the solution by evaporation.

2d. Adding the solution of molybdate in a very thin stream, shaking well.

3d. Great care in the washing and brushing off.

As for the weighing, it is advisable to dry in the vessel repeatedly, and weigh two or three times before deciding the weight finally.

In *Eggertz's original method* there is used for the determination of phosphorus in steel only 1 gramme. The solution of molybdate is prepared from 100 grammes pure molybdic acid to 422 cc. ammonia (0·95 sp. gr.), and 1250 cc. nitric acid (1·2 sp. gr.). By using such a small quantity of steel one advantage is gained, viz., that no evaporation after separation of the silica is required, the bulk being very small if the washing has been carefully performed. But, of course, brushing cannot be applied when such a small quantity of steel is taken. Weighed filters have to be used.

As to the precipitation in this case, you may manage to have the iron solution of 15 to 20 cc. volume, and add to the same at least half its volume of the above-mentioned solution of molybdate. The solution is well stirred and left at a temperature of 40°C. for 1 to 3 hours, after which the precipitate is collected on the weighed filter, dried and weighed.

In Sweden it is of great importance to keep the temperature not above 40°C., as some of the very purest Swedish irons and steels contain arsenic, which will come down as a yellow precipitate similar to the phospho-molybdate. At 70°C. this arsenio-molybdate (containing 4·11 per cent. of arsenic) comes down pretty quickly, and, on boiling, it precipitates at once. It is easy to understand how important it must be to avoid estimating the arsenic as phosphorus in, for instance, a Dannemora "Walloon" iron, where the phosphorus may be only 0·01 per cent. or less. If the right temperature be neglected the phosphorus would, perhaps, appear to be 0·02 per cent.; but this would still be serious in such pure material. In fact, it is to be feared that many mistakes on the part of consumers of Swedish steels are committed through overlooking the presence of arsenic.

It falls beyond the limits of this paper to describe Prof. Eggertz's method for separating and determining the arsenic in steel, and I have only to mention that I have not been troubled with arsenic in any of the rail-steels with which I have had to do.

The Burning Method.—As to the method of burning the precipitate above referred to, I may say that I saw it practiced two years ago at

a large works in South Wales, but not until lately have I had time to try it myself. At the works in question it was the practice to put the wet filter containing the precipitate in a platinum crucible, and to place this in the forepart of a hot muffle-furnace, so as to incinerate the filter. A certain correction, as I was told, was made for the expelled ammonia, before taking down the result. However, I have found hitherto that by adding the filter to the precipitate after determination by the brushing method, in the usual way, and putting it all into a weighed porcelain crucible, then carefully charring the filter and burning it over a gentle flame from a Bunsen burner, no very different results are obtained; indeed, in most cases I have found the weight, after burning, only so much *higher* than after brushing, as would correspond with the weight of the filter-ash, and in no case have I found it less than after brushing. It appears that 140°C. is the highest temperature that can be used when only drying of the precipitate is intended, for at higher temperatures the filter paper begins to change. But, as far as the above-named experiments show, it is quite feasible to incinerate the filter without practically altering the composition of the precipitate. A continued series of such experiments will give a list the average of which will show the accuracy obtainable by the burning method.

Magnesia Methods.—In some laboratories it is usual to redissolve the phospho-molybdate obtained, and then to precipitate with magnesia mixture. This is a rather slow and wasteful way of procedure, and the direct (Riley's) method seems then preferable.

In the direct method one may use at least 10 grammes of steel. The hydrochloric acid solution is reduced by means of sulphite of soda, the excess of sulphurous acid is removed by boiling, the solution is then neutralized with ammonia and bromine added, so as to oxidize about 0·3 to 0·4 gramme of iron. The sesquioxide of iron is precipitated by means of acetate of soda, and the whole of the phosphorus in the steel is supposed to be precipitated along with it.

The precipitate is filtered off and dissolved in hydrochloric acid, about 13 grammes of citric acid are added, and the solution is neutralized with ammonia; 20 to 30 drops of magnesia mixture are then added, and some ammonia. The whole should then be left for two days, and be stirred up now and then during that time before it is finally filtered. The precipitate is washed with ammoniacal water, ignited and weighed.

It is difficult to see where the advantage of using this method would lie, as compared with the direct weighing of the phospho-molybdate. The ignited phosphate of magnesia contains 27·95 per cent. of phosphorus, and an error in weighing must have, therefore, a serious influence on the result, unless a very large quantity of the steel be used. But altogether, the magnesia method requires much more time, skill and labor than the molybdic method. To this should be added the statement by Professor Eggertz, that it is exceedingly difficult to obtain the reagents used in the magnesia method free from phosphorus, whereas in the molybdic acid method it is only the nitric acid which may contain phosphorus. Too high results are, therefore, frequently obtained by the magnesia method.

In conclusion, I may say that the above-described brushing method, which I use daily, has given very accurate results, as the same borings have been checked by some of the leading chemists of the day. As to the time required, if several analyses are performed at the same time, and suitable arrangements made, two or three, or even more, results may be obtained in a day.

Silicon Determination.—For determining silicon in rail steel I use the aqua-regia and the sulphuric acid methods. The former has been sufficiently described in connection with the phosphorus determination, and I will here, therefore, only mention the principal details of the latter.

For each gramme of steel I use 14 cc. of a mixture of sulphuric acid and water, in the proportion of 1 of sulphuric acid to 6 of water. If I wish to estimate the silicon only, no oxidizing of the solution is necessary, and I have only to boil (with exclusion of the air as far as possible) until all is dissolved, and then completely evaporate the water so as to render the silica insoluble. The white salt is then taken up with hot water and a few drops of strong hydrochloric acid, and the silica filtered off and washed with hot water containing 5 per cent. of nitric acid.

If manganese is to be estimated in the solution obtained, the solution should be boiled with a few cubic centimetres of nitric acid for about one-quarter of an hour before evaporating down. After dissolving the salt in water and hydrochloric acid, boiling should be continued for another quarter of an hour before filtering off the silica, so as to insure the manganese being converted to manganous oxide. The silica must, in this case, be washed, first with ordinary cold water, and

then with the nitric acid water, which should flow into a separate beaker and not into the first filtrate, where it might produce a higher state of oxidation of the manganese.

I find that the aqua-regia and the sulphuric acid methods yield results which are quite uniform and concordant. The sulphuric acid method can be quite as rapidly used as the aqua-regia method by means of the hot-plate. Two years ago I worked this method in a steel laboratory in Wales, and the chemists at the place took such a liking to it on account of the absence of the disagreeable fumes, which are evolved in the aqua-regia method, that they started working it for the daily determination of silicon in their pigs. At the present time they use the sulphuric acid method exclusively, and are by practice enabled to work it quite as rapidly as the aqua-regia method. Besides being a neater method, the sulphuric acid process effects no inconsiderable economy in daily practice, where otherwise large quantities of aqua-regia must be consumed.

When using acid water, as in the sulphuric acid method, or strong hydrochloric acid, as in the aqua-regia method, for washing the silica, I make no deduction for filter-ash. The best Swedish filter-paper does not leave any practically estimable quantity of ash when treated in that way. Otherwise, a deduction is made according to Eggertz's formula:

$$\text{Ash, grams} = 0{\cdot}0001\ D^2,$$

D being the diameter of the filter.

Manganese Determination.—For determining manganese in rail steel I use the acetate of ammonia and bromine process, with *final additions of ammonia*, as usual in English and Welsh steel laboratories. In Germany, the method with acetate of soda and bromine or chlorine, and *no final addition of a strong base*, is used, and in Sweden the acetate of soda and bromine method, in accordance with Prof. Eggertz's directions. In describing my mode of operating, I will also try to point out the great differences between the method with *ammoniacal salts and bromine and ammonia combined*, and the methods with *fixed alkaline salts;* and to show what are the precautions to be taken in each case to attain accuracy.

Bromine and Ammonia Process.—3 grammes of steel are dissolved in a flask of 1 litre capacity by aid of aqua-regia; the solution is boiled down, and finally dried. The mass is then dissolved in hydrochloric acid by boiling; water is added to about 750 cc. volume, and the solu-

tion neutralized with ammonia or carbonate of ammonia. If too much ammonia is added, care must be taken immediately to add some hydrochloric acid and to boil for a short time, so as to prevent manganese being precipitated. When neutralizing is completed add 20 to 30 cc. of strongly concentrated, thick acetate of ammonia, and boil until you see the precipitate settle clear after lifting the flask off from the lamp. If the supernatant liquid will not become clear, add cautiously a few drops of strong ammonia (0·88), shake the flask and boil for a moment again. In this way you are certain to obtain a clear supernatant liquid; but you must be very careful not to add too much ammonia, as the manganese may then be partly precipitated as hydrated oxide. After settling, the clear liquid is passed through a filter of 10 inches diameter into a large flask, and finally the precipitate of basic acetate of oxide of iron is poured on to the filter and the remainder of the fluid allowed to filter well off. When no more drops seem to come from the funnel the basic acetate is washed down into the first flask by means of boiling water, and hydrochloric acid is added. The flask is well shaken and heated to boiling, in order to insure the remainder of the manganese being present only as manganous oxide. Neutralizing and precipitation is then repeated as before, and the filtrate added to the first one obtained. For rail steel I find two precipitations like these quite sufficient, the manganese in such steels rarely exceeding 1 per cent. But for spiegeleisen, ferro-manganese, etc., it is certainly desirable to redissolve twice, as the more manganese there is in the substance the more of it will be retained in the iron precipitate. Anyhow, it should be borne in mind that a good boiling is necessary after every re-solution, in order to convert the manganese to *manganous* oxide.

The collected filtrates contained in the large flask are then allowed to cool (this takes only a short time, the first filtrate cooling the second, and so on), about 4 cc. of bromine are added and the flask well shaken, so that the fluid may be well saturated with bromine. It is the safest always to add so much bromine as to have quite a reddish color in the solution. Ammonia (0·88) is then added in excess, and the flask well shaken. At first the solution generally becomes quite colorless, but after continued shaking the brown color begins to be more and more evident, and soon the oxide of manganese separates in lumps. It is then boiled for a few minutes, the precipitate allowed to settle and then filtered off, washed with hot water, dried, ignited and weighed.

It is necessary to have the solution quite cold and a large excess of bromine present when precipitating the manganese in this way. If the solution be hot a violent evolution of nitrogen gas will take place, and the manganese remains in the solution. Insufficient bromine also causes only a partial precipitation. One may write the reactions in this process as follows:

I. *Insufficient bromine:*

$$2[MnO_2(C_2H_3O)_2]+5Br+8NH_3+(x+2)H_2O=(MnO_2+xH_2O)+MnO_2(C_2H_3O)_2+5NH_4Br+2[NH_4O(C_2H_3O)].$$

II. *Sufficient bromine:*

$$MnO_2(C_2H_3O)_2+5Br+8NH_3+(x+2)H_2O=(MnO_2+xH_2O)+5NH_4Br+2[NH_4O(C_2H_3O)]+N.$$

(To be continued.)

DISCUSSION ON STEEL RAILS.

Before the American Institute of Mining Engineers, at the Virginia Meeting, May, 1881.

C. P. SANDBERG, London, Eng.:* I think we should all be grateful to the Pennsylvania Railroad Company, and to their chemist, Dr. Dudley, for spending so much time and money in order to solve an important question; nor are we less indebted to the public spirit which leads them to impart the experience thus gained. In Europe no company or private individual has hitherto done anything similar. In England the great railway companies employ their engineers in other ways, and do not keep a chemist or specialist to study rails; but even if they did they would probably not publish the results. The chief professional engineers are so occupied with their private practice of railroad construction generally, that they cannot be expected to devote their time and energies in so special a question. The German Railway Union has, in my opinion, missed the real object of their elaborate researches by falling into the error of specifying costly impracticable tests for rails—tests at the same time which do not infallibly expose the impurities which may be in the rails under examination. There has, therefore, been nothing done on this side of the Atlantic in the way of exhaustive study to determine the best composition for steel rails. America should consequently have full credit for the enlightened example which she has set.

* Sent to the Secretary in manuscript.

I wish, however, to make a few remarks on Dr. Dudley's second paper—a paper which follows very much upon the same lines of argument as the first, read three years ago, upon which I have previously touched in my paper on the same subject read at the meeting in August last. If Dr. Dudley had discovered a new metal he could hardly, I think, have taken more pains to prove that he was correct in his first formula for the best chemical composition of steel rails. But I have seen so much done in the way of proof by experiment that I am inclined to think that almost anything can be proved by experiments and by samples. If Dr. Dudley would only give up his formula, which would not suit any country—not even America—to work by the ordinary Bessemer process, and would satisfy himself by applying such chemical tests in comparison with mechanical or physical tests, which are practically workable, with a view to arrive at something better in rails generally, he would, I think, have done his company and society at large better service than even he has so far rendered. As it is, he seems to have the American makers arrayed against him, and I fear that the European makers will not sympathize with him very much either. No one can, however, deny that Dr. Dudley has carefully worked out a series of experiments, sparing no pains to substantiate his views. Still it by no means follows that his conclusions are correct, and, as has often happened before, doctors may honestly disagree as to the best means of arriving at the truth.

As to the discussion at the Philadelphia meeting, it is to be regretted that it chiefly represents one side, viz., the makers; for the engineers do not seem to have been well represented at the discussion. If they had been better represented the general conclusion would no doubt have been different. Some of them would probably have agreed with Dr. Dudley in many respects where the makers were against him; for instance, they would have held that both copper and sulphur are of more importance to the producer, and, therefore, the consumer can safely leave them out of the question without detriment to himself.

But for blooms it will be necessary, at least, occasionally to determine the sulphur so as to detect red-shortness which might otherwise cause cracks and wasters in rolling out the English blooms in America. This would be particularly the case if the mills used for that purpose had low speed, so as to prevent the steel being worked out at

a good heat. As for copper, an excess is more rarely the case; moreover, a larger amount may be tolerated than in the case of sulphur, so that this determination is not of such importance as that of the sulphur. In the basic process the sulphur is of very great importance, as the ordinary white pig iron generally contains a greater amount of this impurity than does the Bessemer pig iron. It is, however, to be hoped that by increased experience makers will succeed in the elimination of sulphur as well as they have done with the phosphorus. Anyhow, sulphur will always be a source of trouble to the maker rather than to the consumer of rails.

The carbon test by coloration according to Eggertz's method is quite reliable within any limits that occur in rail-steel. It is true that it does not show the graphite, but the amount of this is so small that it can be left out without any detriment in practice. Besides, this method is very generally adopted, and where the make is large and every charge has to be tested, the number of tests may amount to about one hundred a day. How could this be done if combustion tests were used? Moreover, I should mention that Professor Eggertz has lately introduced considerable improvements, by which greater accuracy can be obtained even when the carbon is very low.

No one has a right to doubt the correctness of analytical results made by Dr. Dudley's assistant as long as he certifies to them. But the makers have, no doubt, some right to complain of the small amount of silicon allowed in Dr. Dudley's formula, viz., 0·4 per cent. What would Dr. Dudley say if, on analyzing some steel rails from his own road, which had given very good results, he had found ten times as much silicon as his formula required—a result which I think quite possible, as I have sometimes found this amount of silicon in steel of excellent physical character. There is no impurity in steel rails which may vary so much as the silicon, not only in different districts and countries, but even in the same works. It often varies four or five times as much at one time as at another, the variation depending chiefly upon the heat of the blow (the pig iron used being the same). Silicon passes away simultaneously with the carbon in the cold blow, but keeps in almost undiminished quantity in the hot blow; and the makers have as yet no means of determining when the silicon is removed, as they have, for instance, in the case of carbon. The use of the spectroscope has been of but little value in practice on a large

scale. It would therefore be very hard—in fact, quite unnecessarily severe—to limit this impurity to such a degree as to be scarcely attainable in practice.

I fear also that Dr. Dudley's physical tests are quite as impracticable as his chemical ones. To apply a bending or a shearing or torsion test to an article which has in daily use to sustain concussion and abrasion seems a curious course of procedure. Besides, these tests are too slow and costly to be of much service in actual practice, just as is the case with the tests of tensile strength and contraction of area stipulated by the German Railway Union.

I have had ample experience of late to prove the comparative worthlessness of such tests in Germany, and I should regret to see America or any other country adopt them. For years these tests have been insisted on by the German Railway Union for all their rails; the drop test has been so reduced as to have little or no effect, and even in some cases it has been abolished altogether. Since I commenced inspection at German works, not only have I found some makers objecting to my heavy drop test, but on analyzing the German rail steel I have found that it contains twice as much phosphorus and silicon as the English rail steel, where the drop test has principally been used for many years. In fact, the scientific German tests, with all their disadvantage of slowness and expense, have proved altogether less efficacious in keeping out these impurities than the simple drop test has been, although the latter is designated as "crude." Still Dr. Dudley recommends the former when he cannot put in execution the still-born idea of a registering manometer on the punching machine for measuring the hardness of the metal, as suggested at Barrow many years ago, but never actually put in practice. Why should we give up a test that has for years done good service for millions of tons, simply because it is "crude," and adopt a slow, costly and impracticable one (which has not done good service), simply because it is "scientific"?

As to the theory of the softest rail being best for wear, I should require further proof on thousands of tons before positively accepting it. If the Pennsylvania Railroad Company and Dr. Dudley will, as we hope, continue their researches on steel rails, not only for their own benefit, but also for the good of mankind, I would venture to suggest that, instead of repeating for the third time these costly and elaborate, not to say tedious experiments, they should lay a thousand

tons of soft rails, made to Dr. Dudley's formula and proposed physical test, and also, on the same line of road opposite, another thousand tons of hard rails, with the ordinary chemical composition, inspected as I have described, so as to compare the results of wear, with the view of proving which is the best. This would cost little or nothing, and after all be more convincing than any experiments, however carefully made, on single bars, such as those now executed with so much care. The only drawback to my suggestion is the time it would require for the comparison, this time depending upon the amount of traffic on the line where they are put down. Meanwhile some approximate results from single rails could be obtained, as to the wearing resistance of soft and hard rails, and the hardness derived either from carbon, phosphorus or silicon, if a locomotive engine were placed on such experimental rails, one being soft and one hard; it should have its driving-wheels sliding on the same spot, water and sand being applied, and the cutting or wear in the two rail-heads could be measured, or the reduction of weight ascertained. This mode would have the advantage of giving results in a day or two, but I admit that it would be but a crude test of single bars.

Ten years ago a paper was read at the Institution of Civil Engineers by a Mr. Price, of Dublin, on a rail-testing machine very much like a turn-table, which was turned round on experimental rails with high velocity, in order to ascertain the resistance against wear for rolling weight; but this machine never came to work, at least not in London, where it was meant to establish it as a public testing-machine for rails. The best testing-machine is now the underground railway, where steel rails of a very heavy bull-headed section last but a few years; but it is doubtful whether the engineers in charge would allow it to be used for experiments, at least they have not done so yet. The best authority on this subject is Mr. R. Price Williams, M.I.C.E., who has read several papers, and collected data and statistics on the wearing resistance of the rails on English railways.

In the address to the meeting of the Iron and Steel Institute, which has been just held in London, the President, Mr. J. T. Smith, of Barrow, quotes Mr. Price Williams' experience of steel rails lasting nine times as long as iron rails, but adds that this is probably more often the exception than the rule; and the past President, Mr. Menelaus, thinks the endurance to be only three times that of iron

rails. I have of late years adopted six times, but since the price of steel rails became reduced to that of iron no calculations are needed to show their preference. It is, however, certain that those steel rails from which such excellent results were obtained at first, say ten years ago, were made harder than they are now, and this seems rather to oppose Dr. Dudley's new theory that the softest rail lasts the longest.

An alteration of importance in the second paper is that the Doctor in his third test has adopted the use of crop ends, although he still adheres to bending tests instead of drop tests for the sake of securing a soft material. But the drop test would also secure a soft material if prescribed to a minimum deflection for a certain blow given; and at the same time it is a criterion of safety. I have stated that as an effect of my standard drop tests the deflection should amount to 3″ or 4″ according to the hardness of the steel. I admit that the difference in the foundation, as to solidity, might be misleading, but, at any rate it is good enough for comparative tests made in the same place and for the same section; besides it has the advantage over the proposed mode of cutting out the test-piece from the rail and bending in the machine, a test which is too slow and costly to be used daily in practice.

With a view of securing soft material for rails the basic process comes in most beautifully; by this process they can be made as soft as lead and with only a trace of silicon, and still the ingots are made solid, no doubt for the reason that the basic steel is cast so much hotter than the Bessemer.

In fact, the difficulty by this process was at first to make steel of ordinary hardness; but now, since hematite pig is added instead of spiegel, steel of any degree of hardness can be produced. Indeed I am just now inspecting blooms made by this process, and judging from several analyses made, I have found the average contents as follows: silicon only a trace, carbon, average, 0·33, manganese 0·35, and phosphorus 0·08. Such rails, tested to my standard drop test, show a deflection of about 5 inches, which indicates more softness than ordinary rail steel made by the Bessemer process. The safety from breakage secured by the soft metal is decidedly an advantage to the engineer, but not exactly so to the railmaker, since the softer metal necessarily gives more cracks and wasters than steel of medium hardness.

Having been the inspector to the Swedish government railways for the last twenty years, I have been naturally anxious to secure to them a rail which should be safe against their rigorous climate, and fearing that the so-called phosphor-steel would break, I made, at the Panteg Works, in South Wales, six years ago, some special phosphor-steel rails (about one hundred rails, with the amount of 0·25 per cent. phosphorus, but low in carbon and silicon), and tested them to my standard drop test.

These rails have been down at Stockholm on a siding ever since; none of them have broken, although the cold sometimes reaches minus 30°F. I can also give the experience from the Swedish state railways of other rails, with a composition of carbon, average, 0·20 to 0·30 per cent., phosphorus 0·06 to 0·12 and silicon 0·10 to 0·30 per cent. Out of 35,000 only four rails broke during the winter of 1880, and these breakages might be equally well caused by mechanical force as by the chemical composition. However, it is of course safer for cold climates to have as little phosphorus as possible in the rails, and to secure hardness by the presence of carbon.

The results just given show how wide a variation in the composition can be tolerated, even for rails exposed to a most rigorous climate. I think we ought to be indebted to Dr. Dudley for having raised the question of chemical composition, for the more we go into the matter the more our views will be widened, and we shall be disposed to grant more liberal variations in the impurities than we should have done if we had not analyzed the metal. Before chemistry has enlightened our views, there will naturally be errors committed from "*trop de zèle*" on the part of the engineers. For instance, an eminent engineer, not long ago, rejected some steel rails because they contained 0·15 per cent. of silicon, although they were faultless in all respects.

Another question arises as to the wearing resistance and hardness derived from carbon alone, compared with that from phosphorus, silicon or manganese, carbon being, in these cases, at a minimum. Whether the wearing resistance is the same or not, we know that safety is best secured by the carbon hardness, and by a minimum amount of the other impurities, particularly in countries with cold climates. However, in other countries with mild climates, and where the ores are impure with phosphorus, millions of tons of rails have been made, and laid down on the track, which seem to answer well,

both as to safety and wear, although the carbon must be low, with a rather high proportion of phosphorus. Since, by the discovery of the basic process, the phosphorus can be eliminated and the carbon added by using pure hematite pig instead of spiegel after the blow, there is an opening even for the countries with hard climate, and with impure ore, to produce good rail steel with a minimum of phosphorus, and with hardness resulting from carbon, just as if the steel had been made originally from pure ores. I should also remark that the amount of manganese—0·35—given according to Dr. Dudley, might well be doubled as a means of lessening the other "deadly sins of impurities" —to use Dr. Siemens' expression—and also in order to facilitate rolling into clean rails.

I would sum up my remarks on Dr. Dudley's paper by saying that, while I disagree with his tests, both chemical and physical, I nevertheless agree with him in the application of chemistry to steel inspection; but it must be used only in a practical way; and if I have, in my paper read before this Institute, shown any decided preference for mechanical tests, it is simply because they have, under ordinary circumstances, done good service, and proved quite sufficient in practice. I have also said, "Let the chemist help us, but not be our dictator." For my own part, I apply chemical combined with mechanical tests, and I have in that way even carried out inspection under Dr. Dudley's specification where railway companies insisted upon adopting it. Mechanical tests come first, and the drop test to my standard is insisted upon for both rails and blooms (sample blooms are rolled into rails for the purpose of being tested); borings are then taken from the steel actually tested mechanically, and sent to my laboratory, where the carbon, phosphorus, silicon and manganese are determined.

However, I do not think it advisable to publish any particular formula, or maximum and minimum of composition, because we are really not yet in a position sufficiently assured to do so. I think the first step in introducing chemistry into rail and bloom inspection is to institute a combination and comparison between chemical and mechanical tests. After sufficient experience and reliable data have been obtained the time will have arrived when exact formulas may be established.

In the inspection of rails I only analyze when the mechanical test gives any extraordinary results. If, for instance, a rail has

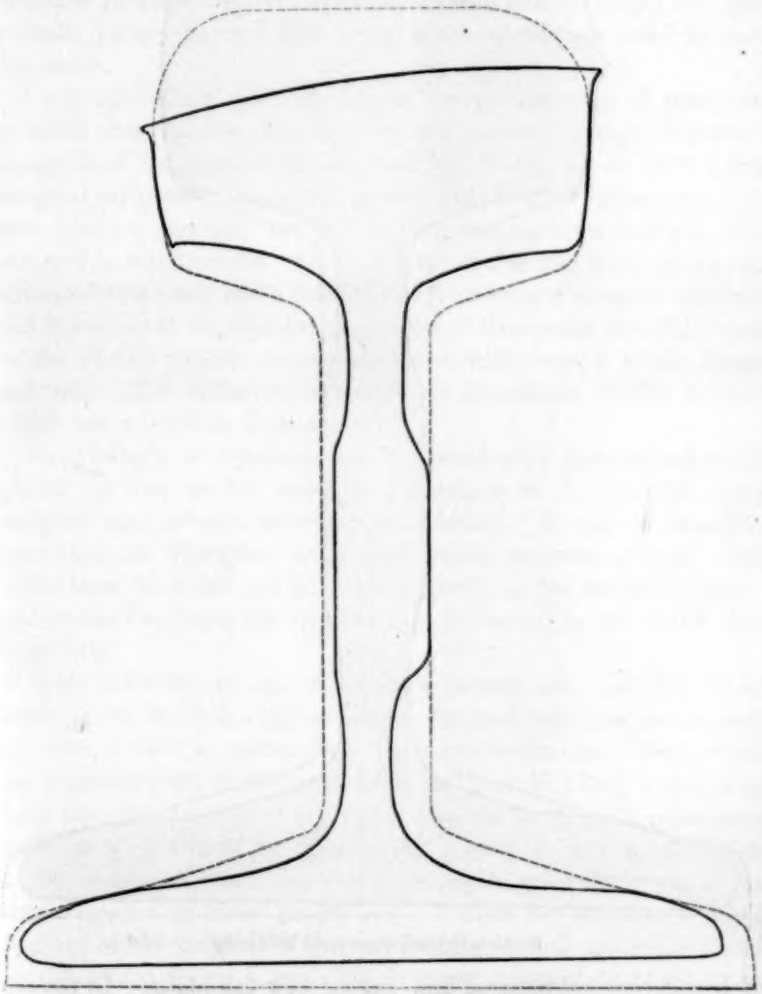
broken under the falling test, if it has broken on the road under service, if it bends differently, showing different degrees of hardness, then the chemical analyses are of interest, and may be of service. After all, however, it is only a large number of trials that should count, and no decided judgment should be formed from single observations. Chemistry has been very useful to the maker, and may be equally so to the engineer when properly applied; but no positive deductions should be made except after large and varied experience and experiments.

I think I have shown that, as a professional inspector, I have, at any rate, done my part towards introducing chemistry into my practice wherever railway companies and my clients specify such tests. For their own sakes, as well as for the solution of many questions now under discussion, it is to be hoped that purchasers, without specifying too much, will rather proceed by degrees to adopt chemical inspection of their rails and blooms, in order to avoid too great conflict. The engineers should never lose sight of the fact that the solution of such a question as this would be arrived at much more safely, and much better, with the assistance of the makers, and therefore it is not expedient to vexatiously oppose them. On the other hand, the makers should bear in mind that they cannot have it all their own way, and I may safely say that they have *not* got it in Europe.

I agree, without reservation, with the views which my friend, Mr. A. L. Holley has expressed in his paper on "Rail Patterns"; and I can prove, by the contents of my circulars, and by the correspondence in technical papers, which has from time to time taken place, that the opinions I held when I took up the question of standard or normal patterns have remained unchanged, and are identical with those referred to above. It is, however, evident that occasional remarks on the subject, published here and there in various ways, are not so valuable and serviceable as when collected and issued in a complete and concise form, and it is such a work of reference on the subject of rail patterns that is now available in Mr. Holley's paper.

Rail making is a comparatively new branch of the iron and steel industry, yet it is of great importance, and railmakers have every inducement to improve their machinery, so that in course of time more difficult sections will be rolled with ease, until ultimately, perhaps, a thinness will be obtained comparable to a spider's web.

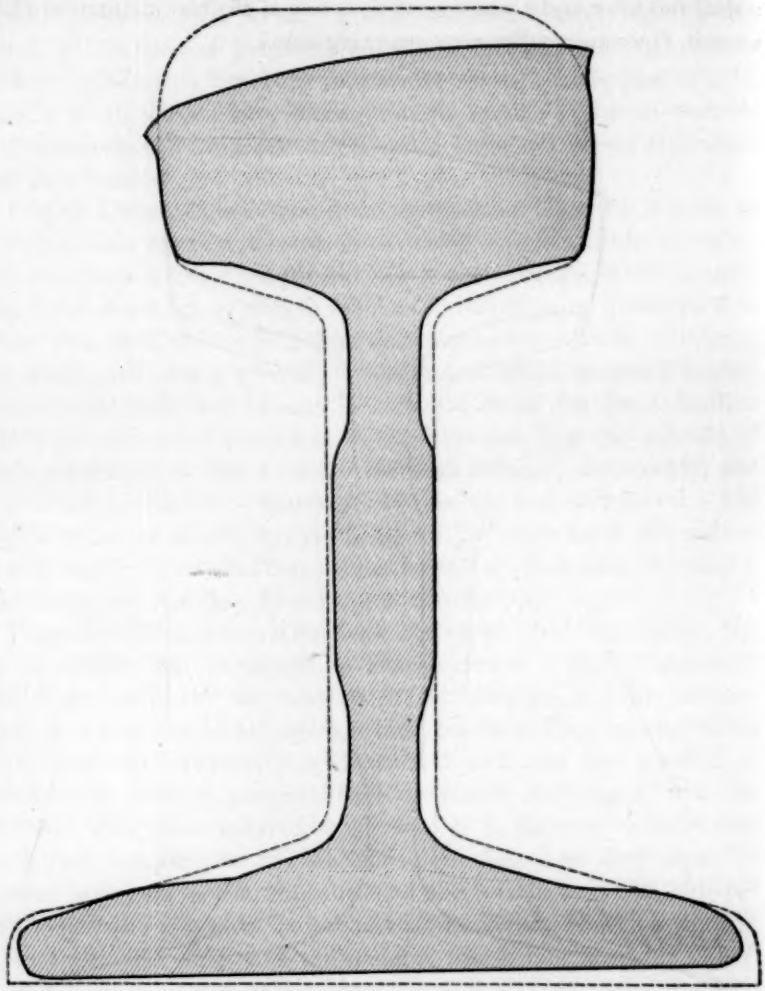
On the other hand, just to show that there is some "body" required even for the flange and web of the rail to resist the wear and tear at the joint, particularly if it is loose, I inclose a lithograph drawing of a steel rail after eight years' service on one of the best maintained railways in Germany. (See accompanying cuts.)



End View.

But it necessarily follows that, with this progression, normal forms and patterns will only be retained as standards for periods of greater

or less duration; and it will become a question for the consideration of the railway engineer whether it would be more advisable to retain and alter what he has, or to put up with the inconvenience



Section beyond the second bolt-hole.

of keeping several patterns and having new fish-plates. In the construction of new lines one can choose the most modern and economical section, only limited by the price at which it can be obtained. Under these circumstances, it is evident that no patterns will be uni-

versally adopted forever, notwithstanding the vast amount which would be saved if they could possibly be definitely fixed. It is curious to remark that the very man who now proposes to finally settle upon a standard is, probably, the one who by his genius, aided by the immense resources of the United States, will produce more desirable patterns and sections than those which he would now like to make permanent, and thus bring about the changes which he now deprecates.

I willingly admit that, considering the present stage of excellence to which rail making has attained, my pattern appears "clumsy" alongside of the more difficult one of Mr. Holley's. In 1870 I first designed my pattern in accordance with what English railmakers could then produce, and with the end in view that no extra cost was to be incurred in manufacture. In 1878 I found that the mills were so far advanced that there was a decided call for a second series of patterns, and it was not to be expected that either of these series should be equal to the Holley pattern, recently designed, with respect to the flanges and web. The difference is merely an illustration of the progress which has meanwhile been made.

Mr. Holley's new patterns are produced after the vast experience gained by him in his visits to railmakers in Europe for newly designed and constructed works in America. It may be remarked here that the European mills have much improved of late. New mills have been laid out in England, partly on the reversing system, and on the Continent the improvement is marked by the use of three high rolls.

With reference to Mr. A. Welch's remark and also Mr. Hart's letter, about the first original section for steel rails designed as early as 1866, I wish to assure both these gentlemen that I had not seen the patterns when I designed mine, and even if I had, I should not have then dared to put it forth as a standard for English railmakers; and the soundness of my opinion has a simple but conclusive proof in the statement made concerning the extra price which was at that time required for their production. I need not say that I highly approve of the original Welch section, for it is in principle similar to the design which I made fifteen years subsequently; and from a professional point of view I would fully concede to Mr. Welch the credit of first designing the modern steel rail section. From a commercial, matter of fact and practical point of view, he was, however,

before his time by so many years; or, to put it in another way, rail making was ten years behind its time when the new metal steel was first introduced for rails.

If his design has been incorrectly identified with my name (as the sections are similar in principle, though they differ in thickness of flange and web), I beg to say (1st) that I much regret the misunderstanding which has so arisen through no fault of mine, and (2d) that no confusion has been intentionally created by me.

It is a pity that the section designed so long ago has not been before made public, as it would make a really good standard pattern, and even now quite equal to present requirements.

In conclusion, I wish to say that I have never desired to claim any invention or novelty by the publication of my sections; they were put forth as being such as English railmakers could and would produce at the ordinary market price, and as being the best and most suitable to be so obtained. Had Mr. Welch or Mr. Holley put forward theirs ten or fifteen years ago, I would have been the first to recommend them. In fact, I should have been glad to see their names associated with rail patterns in their own country, in the same way as mine has been.

Prof. Rich. Akerman, Stockholm, Sweden:* I agree with your (Mr. Sandberg's) views, that it is too early yet to stipulate only one certain chemical composition in the rails. Such a stipulation, I think, might be justified, if it was not for the manganese; but this metal changes the properties of the iron and steel in the same direction as carbon, silicon and phosphorus, by increasing its tensile strength, stiffness, hardness and brittleness. If the iron and steel does not only contain carbon and manganese, but also phosphorus, silicon and sulphur, I think everybody will agree that the manganese, to a great extent, can neutralize not only the effect to red-shortness, caused by the sulphur, but also the tendency of the iron or steel to become burnt or brittle, caused by the phosphorus and silicon. Thus brittleness can be as well increased as decreased by the presence of manganese, and I, therefore, consider that, however necessary analyses now and then may be, in order to enlighten us and to explain exceptional cases, yet they are not convenient as practical tests upon rails, especially as the properties of the rails not only depend on the chemical composition of

* From a letter to Mr. Sandberg.

the iron or steel, but to a great extent also upon the soundness of the ingots and the mode of manufacture.

Still less necessary than analyses I regard the German system of controlling the rails by researches on contraction of area. I do not consider either the elongation or the contraction of area, in fracturing samples, to test tensile strength to give a correct idea of the toughness of iron or steel. On the contrary, I regard the falling test, properly carried out, to be much better in this respect and to give a greater safety against breakage than the common tests on tensile strength.

DR. R. W. RAYMOND, New York: It occurred to me after the presentation of Dr. Dudley's interesting paper on this subject, and after the discussion to which it gave rise, that the important data he had collected might be made, by suitable mathematical treatment, to yield more significant indications than had yet been obtained from them. It is true that Mr. Cloud has discussed by the method of least squares, the results set forth in Dr. Dudley's former paper, on the breakage of rails. But the breakage of a rail is, I think, far more likely to be due to mechanical or accidental (not chemical) conditions than the loss of metal by wear. For this reason I should attach less importance to the data, and place less reliance in their discussion. Moreover, the number of observations in the paper referred to was relatively small—too small to permit the method of least squares to give trustworthy results. Finally Mr. Cloud correlated the chemical constitution with certain physical tests, not directly expressive of the resistance of the rail to breakage. In the case before us we have sixty-four observations, and they directly connect certain chemical proportions with a definite physical result of experience, namely, the loss of metal by wear. I think, therefore, that this case presents a better opportunity for such a mathematical discussion as will test, to some extent, the assumptions underlying Dr. Dudley's conclusions.

These assumptions are: that the loss of metal per million tons of traffic depends, first upon the circumstance under which the rail is worn; secondly, upon the proportions of its various constituents, and that the amount of loss is affected by each constituent in a certain ratio, so that if the circumstances were the same for all the rails, the wear per million tons might be approximately expressed by an equa-

tion of the form $I+Cc+Pp+Ss+Mm=R$, in which I is an unkown value, depending on the quantity of iron (and other ingredients not determined) in the rail, and assumed to be constant for all the rails; c, p, s and m are the experimentally determined quantities of carbon, phosphorus, sulphur and manganese, as tabulated in Plates VI and VII of Dr. Dudley's paper; and R is the experimentally determined wear per million tons; while C, P, S and M are unknown coefficients. In other words, it is assumed that the difference in wear is proportional to a difference in c, p, s or m, and that the effects of a change in the amount of either are independent of the amount of the rest—within the experimental limits of the problem.

It is also assumed that sulphur, copper and other ingredients, not determined, are equally present in all the rails, or at least do not affect the wear, since I is taken as constant.

Dr. Dudley tacitly assumes the coefficients C, P, S and M to bo positive; and assigns certain values to them, by means of which he reduces the equation to the form $I+Px=R$, x being equal to $\frac{Cc+Ss+Mm+Pp}{P}$, or the number of so-called phosphorus units in the rail.

But we do not have R directly given under constant circumstances. The varying weights (per engine, car or train) and the varying speeds we must neglect for lack of data, and assume that in these respects the rails examined have been treated substantially alike. But there remain the varying conditions as to track, which must be eliminated. Dr. Dudley gives six classes, four of which contain 8 rails each, and two 16 rails each. Beginning with the first (tangent grade), we take the mean of the figures expressing wear for that class; and continuing this process through the other five, we obtain six expressions in quantity of loss by wear, which we assume to represent, in their differences, the varying conditions of the track, with respect to grade and direction. We now take the mean of these means (giving double weight to the groups containing 16 observations). This general mean is the loss of metal per million tons of traffic for a rail of average constitution under mean circumstances. The difference between it and the mean for any one group is a constant correction to be applied to the tabular wear of each rail in that group, to obtain what may be called the reduced loss for that rail; that is to say, an expression for the loss per million tons of traffic which that rail

would have shown, had it been worn under average circumstances. For example (the tabular numbers being multiplied by 1000, to get rid of long decimals), the general mean of loss is 76·7; the mean for the tangent-grade group is 70·1; the correction is +6·6; the experimentally determined loss of rail 887 (of that group) is 38·6; and the reduced loss, 45·2.

I had at first intended to do with the units of wear what Dr. Dudley did with his chemical units—reduce them all to one actual group. As he obtained phosphorus units, so we might obtain tangent-grade, or grade-curve or level-curve units. But the reduction to an ideal average unit of wear is preferable, as giving the smallest possible average corrections, and distributing the errors involved in the corrections uniformly over all the observed cases, instead of concentrating them on a part. I am indebted for this suggestion, and for the larger part of the voluminous calculations required in the details of the work, to a mathematical friend, without whose aid I should scarcely have been able, in the scanty intervals of other absorbing occupations, to solve the problem, even after stating it.

It is, however, simple enough, though very tedious. From the 64 given cases, we form 64 equations of condition, of the form $I + Cc + Pp + Ss + Mm = R$, in which the different symbols have the same meaning as before, except that R is the reduced loss by wear, as already explained.

These 64 additional equations are now, by the method of least squares, reduced to 5 normal equations, containing the five unknown quantities, I, C, P, S and M. The solution of these equations will give us the most probable values for I, C, P, S and M which the equations of condition can yield.

I do not intend at this time to publish the details of the calculation. If time permits, there are other assumptions which I should like to introduce into the discussion, to test the results already obtained. It would be interesting, for instance, to repeat the whole process with equations of the form $I \times Cc \times Pp \times Ss \times Mm = R$.

At all events I am not now prepared to present the figures of the calculation, in which, though the equations have been solved, the probable errors of the solutions have not yet all been determined. But I am able to say that Dr. Dudley's own data and assumptions, thus treated, show the coefficient of silicon to be negative, and that of manganese to be practically zero. In other words, the silicon,

though a "hardener," does not, like carbon and phosphorus, increase, within the limits of these tests, the loss of metal by wear; on the contrary, it increases the wearing capacity. And manganese seems to be neutral in that respect, behaving like so much iron. These two metals play, then, a very different part from that assigned to them in Dr. Dudley's formula, which holds true (and that with modified coefficients) for the non-metals, carbon and phosphorus only.

I will not here enlarge upon the imperfections inherent in the application of the method of least squares, and in the necessary employment of so many assumptions. In spite of them all, I believe that the indications here afforded are significant. Certainly the "phosphorus-unit" system must be abandoned, so far as silicon and manganese are concerned; but are there not hints of practice pointing in the same direction, as the results of this mathematical inquiry, as to those two elements? Have we not heard repeatedly of high-silicon rails showing extraordinary wear? And is it not "important, if true," that manganese may be increased beyond Dr. Dudley's limit, if it be otherwise useful or convenient, without injury to the wearing capacity?

Dr. C. B. Dudley said that he had listened with much interest to the present discussion, but he was not prepared to reply at the moment to all that had been said in criticism of his paper read at the Philadelphia meeting. At a subsequent meeting, after he had had opportunity to carefully examine and study the remarks of the different contributors to the discussion, he would briefly sum up the case from his standpoint.

New Model for Vessels.—Prof. Pictet is experimenting with a new model with the view of obtaining a great increase of speed at sea. The keel is so shaped as to reduce the resistance of the water, and the vessels, instead of sinking their prows when the velocity increases, raise them so as to diminish the friction and glide over the water. He expects to attain a speed with steamboats of more than 30 miles an hour. A boat is now in process of construction at Geneva, and experiments will be made with it upon the lake.—*Les Mondes.* C.

NECESSITY OF CLEAR MECHANICAL CONCEPTIONS.

By WILLIAM D. MARKS,
Whitney Professor of Dynamical Engineering, University of Pennsylvania.

The nomenclature of mechanical conceptions is a matter of common consent, and could soon be reduced to approximate uniformity were the majority of writers upon mechanics sufficiently painstaking and clear in their use of terms, and would they bear in mind that a clear physical conception of the meaning of terms used is of vastly greater importance than any subsequent display of skill and ingenuity in the mathematical manipulation of the symbolical expressions for them.

Every term used in mechanics should convey to the mind a distinct physical conception, capable of being expressed in intelligible language without recourse to symbolical notation; and until this fact is recognized and acted upon, mechanics will ever be a dreaded study to those who are forced to take it up, saving that small proportion of students thoughtful and patient enough to elaborate their own conceptions by careful decomposition and isolation of the elements of the symbolical expressions which are taken for the foundation stones of an elaborate mathematical structure.

None who are engaged in teaching can have failed to perceive the stupefying effects of a course of symbolical reasoning unaccompanied by any attempt to materialize the meaning of the expressions deduced, or have not noted the injury of a naturally clear intellect in the attempt to memorize a mass of partially apprehended formulæ.

Great mathematical acquirements do not seem to be an absolute necessity, since discoveries in natural philosophy seem to point out new and appropriate methods of quantitative treatment rather than to render available the labors of the pure mathematicians.

Our knowledge of mathematics does but enable us to weigh and measure our results. Every new mechanical problem should first be analyzed by means of a course of abstract reasoning before being quantitatively analyzed with the aid of mathematics, just as a cautious chemist precedes a quantitative analysis by qualitatively determining the nature and ingredients of the substance under consideration.

Clear ideas of the meanings of mechanical terms are imperatively

required as a first condition of success in a preliminary analysis of any problem, and any attempt at quantitative analysis will more probably lead to error than truth unless this preliminary analysis be complete.

That the writer may not appear to have "set up a man of straw" for the pleasure of demolishing him, he will instance a few cases occurring in the works of the abler writers upon mechanics, passing over without notice the too apparent evasions and misconceptions of a host of writers of so-called "elementary mechanics."

Prof. Wm. Whewell, who is probably the clearest writer in the English language upon mechanics, constantly uses the terms force and and pressure interchangeably. Pressure refers rather to force distributed over a considerable surface, as in the case of water, the atmosphere, etc.

Prof. Rankine calls the moment of inertia of a revolving body the weight multiplied by the square of the radius of gyration; this expression is not the moment of inertia, but only *the measure of the moment of inertia.*

Prof. Tyndall defines heat as "a mode of motion"; it is really a form of work. Possibly this apparent error is a wilful misstatement, made with a design to convey to his readers an approximate idea of what he did not believe them capable of conceiving fully. It certainly is either an error or a concession to ignorance, which has done much harm.

It is very much easier to criticize defects than to remedy them, and the author, in offering the following verbal definitions, does not feel that he has made himself as clear as he could have wished to be.

He trusts, however, that they will serve the purpose of showing more clearly the meaning of the usual terms of mechanics, and their relations to each other, giving in the present form of successive aphorisms a connected view of the whole field of mechanics.

In order to be perfectly clear, and establish a complete understanding between our readers and ourselves, we will have to repeat the most elementary ideas, because the terms having the more complex meanings will demand the most precise accord as to the meaning of the elementary terms to which they will be reduced. We may, then, be pardoned for the repetition of definitions with which all are assumed to be familiar.

Dynamics may be separated into two studies—kinematics and

statics. When these two are considered in conjunction we have dynamics.

The careful isolation of these two branches of dynamics, and their separate study, will add much to the power of apprehension of the student when he comes to consider them conjointly.

Kinematics evades all questions of force, and in it we confine ourselves entirely to the consideration of the path, velocity and direction of motion.

Motion can best be defined as a change of position, and in many cases the velocity of this change is a matter of indifference, so that the path and direction of motion only receive our consideration. If velocity is taken into consideration we introduce the element of time, since the velocity of a point is the distance which is [or would be if the velocity was constant] passed over in whatever unit of time is used as a standard. One second is the usual standard.

We can then say in uniform motion the space described in any time is equal to the product of the velocity and the time.

When the velocity is not constant it can no longer be measured by the quotient of the space by the time, since these quotients will be different for different periods, and in variable velocities we measure the velocity, at any instant, by the space which would have been passed over in the succeeding second had the velocity been rendered constant at that instant.

Angular velocity, which is used to compare the speeds of rotation of bodies around their axes, can also be constant or variable; it is the velocity, in a circular path, of a point which is at a radial distance equal to unity from the axis of rotation of any rotating body; or, if the axis does not pass through the body, it is the velocity, in a circular path, of a point situated at a distance from the axis equal to unity, and in a an assumed line joining the axis and the body revolving around it. It can always be obtained by dividing the curvilinear velocity of any point in a rotating or revolving body by its radial distance from the axis.

Equipped with these few fundamental conceptions, we have all that are necessary to the study of kinematics, this word being used in its most limited sense.

Statics, on the other hand, evades all questions of motion, and in it we confine ourselves to the study of forces at rest.

What force really is we will probably never know until we learn

the ultimate nature of matter; we do know, however, that whatever tends to produce motion, or actually produces motion in bodies at rest; or brings or tends to bring a moving body to rest, or to change the direction of a moving body, is called force. We measure force by its intensity, in pounds, and limit it by its direction and point of application.

In terrestrial mechanics, to which we limit ourselves in this paper, gravity is the force which attracts all bodies to the surface of the earth in a vertical line; if allowed to act on a free body in a vacuum it will produce a velocity of about 32·2 ft. at the end of one second, during which time the body will have fallen a distance of 16·1 feet.

In order to define the centre of gravity of a body we will have to precede it by the definition of the statical moment of a force which is the intensity of that force multiplied by its perpendicular distance from the point around which it tends to, or actually does produce motion.

We can now define the centre of gravity of a body as that point which, if supported, leaving the body free to rotate in any direction, would balance all the moments of the forces of the molecules of the body due to the force of gravity; the body would not have any tendency to turn about this point, at which the total force of gravity acting upon the body may be assumed to be concentrated.

The well-known theorems of the parallelogram and parallelopipedon of forces form the basis for the statical treatment of forces which has received an enormous development both analytically and graphically.

We come now to dynamics, which is the study of combined force and motion, that is, of work; or if time in which the work is accomplished is included in the consideration, of power.

We can say that work equals force multiplied by the space passed over during the action of the force, and as the unit of space usually assumed is a foot and the unit of force a pound, we measure work in foot-pounds. A foot-pound is the amount of work done in raising a weight of one pound one foot, or in exerting a force of one pound through a distance of one foot in any direction. Work is considered independently of the time in which it is accomplished.

Power is work considered with respect to the time in which it is accomplished; as, for instance, a horse-power, which represents 33,000

foot-pounds of work done in one minute or 550 foot-pounds of work done in one second.

The many terms used to express the idea of force and motion combined can all be seen, by a little thought, to be synonymous with work. Power is often used incorrectly for force when the lever and screw are being discussed.

The weight of a body is the measure of the intensity of the force of gravity acting upon it. In treatises on mechanics it is made equal to the product of its mass by its velocity at the end of one second (32·2 feet) under the action of gravity.

In order to clearly grasp the meaning of this last sentence we must know what mass is. The mass of a body is usually stated to be the quantity of matter in it, and it will at once be perceived that the hypothesis is placed that differences in quantity of matter make proportional differences in the weight, which may or may not be true. We have no means of proving that a volume of iron which weighs 7·2 times as much as the same volume of water contains 7·2 times as much matter.

The fact is that mass means the intensity of the force of gravity divided by the velocity due to the force of gravity at the end of one second, and is a constant ratio at all points on the surface of the earth. The great convenience of this ratio for the purposes of the mechanic will be seen when we recollect that in dynamics the intensity of a force is measured by the velocity which it will produce in one second, and if we multiply this ratio (which is the mass) by the velocity which is observed, we have the intensity of the acting force in pounds. This leads us at once to the momentum of a body, which is the intensity of a constant force which has been (or should have been to produce the same velocity) acting upon it for one second, it is equal to its mass multiplied by its velocity, in feet per second.

If a moving body be brought to rest in one second, the momentum is the intensity of the constant force which must be exerted through a space equal to one-half the velocity of the moving body.

In speaking of momentum, unity of time is always assumed as one of the conditions; thus the weight of a body equals its momentum when gravity is the force acting upon it.

The distinction between momentum and the really acting force in many cases which occur must be sharply drawn. If the acting force be constant, it will add equal increments of velocity in each unit of

time, and the intensity of the acting force can be at once deduced by dividing the momentum by the time of its action. Thus a body let fall and acted upon by the constant force of gravity has at the end of one second a momentum Mg, and at the end of two seconds a momentum of $2Mg$, but the acting constant force is still Mg or $2Mg$, divided by the time two seconds.

Thus we have the distinction, the momentum (Mv) is the intensity of a constant force which acting contrariwise upon any mass in motion, with the velocity v for one second, would bring it to rest; or, if the mass (M) be at rest, will in one second impart to it the velocity v.

On the other hand, the acting constant force $F=\frac{Mv}{t}$ may act for any length of time, t, and therefore may be of any intensity.

Work has already been defined as force (F) multiplied by space (s). If now for F we substitute $\frac{Mv}{t}$ and recollect that for any free body put in motion by the action of a constant force, the space passed over is equal to one-half the final velocity multiplied by the time, we have

$$\text{Work} = Fs = \frac{Mv}{t} \times \frac{vt}{2} = \frac{Mv^2}{2}.$$

This result at once reminds us of the term "*vis viva*," literally meaning living force, and shows us that the work expended in giving motion to any body and stored up in it is one-half the *vis viva*, which can be separated into the two terms (Mv) and $\left(\frac{v}{2}\right)$, the first being the momentum and the second the space through which the momentum will act in one second.

Matter has the power of absorbing and storing up work while being given motion, and gives out work when its motion is retarded. A body in motion and not acted on by any force, will move in a straight line and with a constant velocity. If this body is acted upon by a resistant force, it will not stop until all of the work stored in it $\left(M\frac{v^2}{2}\right)$ is given out.

These phenomena are due to the inertia of matter, meaning by inertia the inability of dead matter to change its position or motion of its own accord, and the passive resistance which it offers to all action upon it. "Action and reaction are ever equal, simultaneous and opposite," but in a free body the reaction is always yielding to the

action of a force in the form of motion of the body in the direction of the acting force, and the matter contained in the body acted upon stores up the combined force and motion as work.

The moment of inertia of a rotating body is the statical moment of the momentum of the body and is equal to the sum of the moments of the momenta of its infinitesimal elements; it is the moment of that constant force with which a rotating body would resist being brought to rest in one second; or, being at rest, would resist having impressed upon it a certain angular velocity (w) in one second. Thus each elementary particle (m) would have a momentum (mv), and a lever arm (r) and its moment of inertia would be mvr; but since $v = rw$ we have $mvr = mr^2w$ as the moment of inertia of each particle and Σmr^2w as the moment of inertia of the whole body.

The definitions of and formulæ for the moment of inertia given in many text-books neglect the angular velocity and the acceleration of gravity (g) and are therefore only comparative measures of the moment of inertia, and do not give a clear conception of what the moment of inertia really is.

If it is desired to know the amount of work done in giving the mass m the velocity v in one second, we have at once the expression $(mv)\left(\frac{v}{2}\right)$ for the work $= \frac{1}{2}mv^2 = mr^2\frac{w^2}{2}$.

A more complete and misleading misnomer than moment of inertia would be hard to imagine if the original inventor of the term meant by it what he is usually assumed to mean, viz., "the work lost or gained whilst the body is experiencing a change in the square of its angular velocity equal to unity," or "the weight of a body which, if concentrated at the distance unity from the axis of rotation, would require the same work to produce a given increase of angular velocity which the actual body requires."

The moment of flexure used in discussions of the elasticity and strength of materials is an analogous expression to the moment of inertia, the modulus of elasticity taking the place of the angular velocity; it is a statical measure but mentioned here because of its similarity to the moment of inertia.

The radius of oscillation of a rotating body is equal to its moment of inertia divided by its momentum; it is the mean radius or lever arm of the momentum; the extremity of this radius is the centre of oscillation or, as it is sometimes called, the centre of percussion of

a suspended body, because if the body be struck at this point no shock will be communicated to its axis; it is the point where statical equilibrium occurs between the momentum of that part of the body between it and the axis of rotation and the momentum of that part of the body outside (away from the axis) of the centre of percussion.

Since the angular velocity is a common factor of the moment of inertia and the momentum of a body, it can be and usually is neglected and the radius of oscillation is found by dividing the measure of the moment of inertia of any body by its statical moment.

The centre of pressure is an analogous term to the centre of oscillation and is found in a similar manner. It is a statical term and its analogy is due to the fact that the pressure of water increases with the depth from the surface in the same ratio that momentum increases with the distance from the axis of rotation, or the elastic resistance of materials increases with the distance from the neutral surface of a beam.

The square of the radius of gyration of a rotating body is equal to its moment of inertia divided by the momentum of the mass supposed to be concentrated at a distance equal to unity from its axis, if the angular velocity which is a factor of both terms be neglected, the square of the radius of gyration can be deduced as usually explained in the books by dividing the measure of the moment of inertia (Σmr^2) by the mass (M) of the body, the square of the radius of gyration (k^2) is the mean of the squares of the radii of the infinitesimal elements of any rotating body. The centre of gyration of any body is that point at which, if its mass were supposed concentrating and revolving with the given angular velocity about the axis, the moment of the momentum of this concentrated body (MVk) would equal the sum of the moments of the momenta of its infinitesimal elements (Σmr^2w) while actually rotating around its axis. Since $k^2 = \frac{\Sigma mr^2w}{\Sigma mw}$ we see that it is the ratio between the actual moment of inertia and the moment of inertia of the mass assumed concentrated at a distance unity from the axis, the centre of gyration has no actual physical existence as has the centre of oscillation, but it serves a convenient purpose in mathematical investigation. From the definition of the radius of oscillation it will at once be seen that it can be deduced by dividing the square of the radius of gyration by the distance of the centre of gravity of any body from its axis of oscillation.

Force is actually exhibited and its action felt only when there is a change in the *velocity* with which a mass is moving. Centrifugal force, which is the intensity with which any body moving in the arc of a circle resists being drawn towards its centre, is a good example of this.

Centrifugal force equals the momentum of a mass multiplied by its angular velocity (*Mvw*).

Generally we can say: In any change of velocity, the intensity of the acting force is measured by the mass multiplied by the velocity which would have been generated in one second had the force remained constant.

The theorem of virtual velocities to which frequent reference is made in works on mechanics, expresses in the form of an equation the equality of the elementary quantities of power being transmitted by any mechanism. Thus, suppose the work actuating any machine to be Fs and the work being done by the machine to be $F_1 s_1$, then letting dt be the differential of the time, we have

$$F\frac{ds}{dt} = F_1\frac{ds}{dt} \text{ or } Fv = F_1 v_1.$$

This theorem may also be interpreted by saying that the power of all parts of a continuous train of mechanism at any instant is the same, frictional losses being neglected.

The question at once arises: What is a machine? To which we would answer that it is "an assemblage of resistant parts," for the purpose of conveying *work* from one point to another by means of predetermined motions.

Many of the laws relating to machinery are incorrectly phrased, and we repeat them in a corrected form:

No machine can give out more work than is put into it.

Losses of work occur in all machinery because of friction.

Lost work due to friction usually disappears as heat.

Whatever is gained in speed is lost in force in all machines.

The work done by every member of a continuous train of mechanism is the same for one cycle.

Perpetual motion, *i. e.*, the creation of work, is not possible.

These laws cannot be dispensed with nor in many cases be deduced one from the other; they are many of them mechanical axioms, and are for the most part the result of experience and of a vast number of experiments never recorded and discussions long since forgotten.

Phila., Oct. 24, 1881.

THE SCIENTIFC PRINCIPLES INVOLVED IN ELECTRIC LIGHTING.

By Prof. W. Grylls Adams, F.R.S.

A series of "Cantor Lectures" delivered before the Society of Arts, London, 1881.

(Continued from page 294.)

The fundamental principles which underlie all magneto-electric machines are the four great principles discovered by Oersted, in 1819, by Ampère and Arago in 1820, and by Faraday in 1831. In 1819, Oersted discovered the action of a current of electricity on a magnet. In 1820, Ampère discovered the action of magnets and currents on currents of electricity in their neighborhood, and in the same year Arago showed that currents of electricity produced magnetization, thus laying the foundations of electro-magnetism; and, in 1831, Faraday showed that induced currents were produced by the motion of magnets. All machines for the conversion of work into electricity are founded on Faraday's great discovery of the induced current, derived from the relative motion of a magnet and a coil of wire.

Magneto-electric machines are divided into two great classes, according as they furnish continuous currents or alternate currents. All such machines are alternate, as far as regards the currents in the coil; but these currents are made to flow always in the same direction in the external circuit, in continuous current machines, by means of a commutator which reverses the contact at every half-turn.

The machines of Pixii, in 1832, followed by those of Saxton and Clarke, were the first continuous current machines. From these we may pass to Wheatstone's introduction, in 1845, of electro-magnets in place of permanent magnets, to produce the magnetic field. In 1854, Messrs. Werner, Siemens and Halske introduced the Siemens armature, in which the coil is wound longitudinally in a groove. The strength of the continuous current depends on the velocity of rotation, on the length of the wire and on the power of the magnetic field formed by the magnets.

It is remarkable that, in 1854, Hjorth originated an idea which was some thirteen years in advance of his time; he patented an improved magneto-electric battery, in which the currents induced in

the revolving armature pass round the electro-magnets and increase their magnetism, and so increase the induced currents at compound interest rate. This was the celebrated principle afterwards re-discovered by Siemens and by Wheatstone simultaneously in 1867, which has formed the basis of all dynamo-electric machines and which, for equal power, are cheaper and more compact than all other magneto-electric machines.

Dynamo-Electric Machines.

In February, 1867, Dr. Siemens and Sir Charles Wheatstone, on the same evening, presented to the Royal Society their two papers, "On the Augmentation of the Power of a Magnet by the Reaction Thereon of Currents Induced by the Magnet Itself." According to the principle then put forward by Dr. Siemens, the rotating armature, the electro-magnet and the external resistance, such as an electric lamp, are joined up so as to form one simple circuit. A small amount of magnetism is communicated to the electro-magnet, so that, on rotating the coil, a current is induced alternately in opposite directions, and after being reduced to the same direction by a commutator, this current passes through the coils of the electro-magnet in such a direction as to make it stronger, so enabling it to react on the armature. Thus the magnet and the armature act and react on one another, strengthening the magnetic field and continually strengthening the induced currents. Sir Charles Wheatstone put forward the same principle, and called attention to the fact that at the first instant of completing the combined circuit the effects are stronger than they are permanently. The principle of these dynamo-electric machines is clearly shown in the Figure 1, where N and S represent the poles of the electro-magnet enclosing the revolving armature, the external resistance in the circuit being represented by an electric lamp. Sir Charles Wheatstone also pointed out that a very remarkable increase of all the effects is observed when a shunt is employed to divert a great portion of the current from the electro-magnet. By that means, four inches of platinum wire, ·0067 in. diameter, was

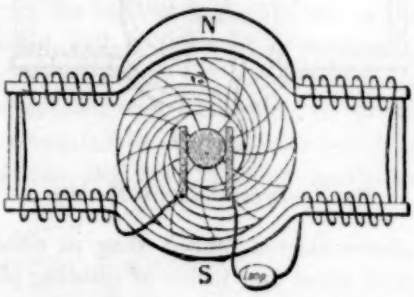


Fig. 1.—Dynamo-Electric Machine.

made to glow. A certain resistance in the shunt was found to be necessary to produce the best effects, so as neither to weaken the magnetism too much nor to give the current too much work to do in heating a high resistance.

In addition to this, Sir Charles Wheatstone showed that the effects above described are far inferior to the effects obtained by placing the work to be done in the shunt circuit. Thus seven inches of wire were made to glow in the shunt, when only four inches of the same wire would glow in the original circuit. There is thus a double advantage, for there is no loss of resistance by introducing the shunt, the resistance of the shunt being the resistance on which the useful work is done.

This improvement, suggested by Sir Charles Wheatstone in 1854, and now being adopted by Dr. C. W. Siemens in his latest dynamo-electric machines, is very well shown in Figure 2, for which, as well as for the other figures illustrating this lecture, I am indebted to the kindness of the Director of *La Lumière Electrique.*

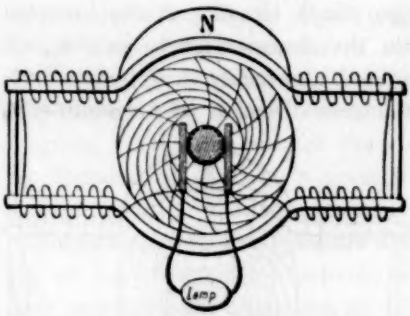


FIG. 2.—Dynamo-Electric Machine.

Wheatstone also showed that the effects are much less influenced by a resistance in the electro-magnet branch than in either of the other branches. Thus, with about four inches of glowing platinum wire in circuit, the addition of about five inches of platinum wire in the armature branch, or in the shunt, produced a $\frac{1}{4}$-inch glow, whilst four feet of the same wire was required in the electro-magnet branch to reduce the glow to three-fourths of an inch. Dr. Siemens has shown that, for the greatest efficiency, the resistance of the rotating coil must be small, but the resistance of the electro-magnet may be increased, and that in both cases the wires should not be small, but of considerable diameter.

THE GRAMME RING.

In the Gramme armature, coils of wire are wound in sections, all in the same direction, round a ring; and each section, when a current is flowing through it, may be regarded as an electro-magnet. The

similar poles of all these sectional electro-magnets will point in the same direction round the ring. Consider only one of these electro-magnets with its north pole directed towards the south pole of another magnet, it will be attracted towards it, and with greater and greater force the nearer it approaches; on passing the south pole, its own south pole will be presented to the south pole of the fixed magnet which it has just passed, and its motion will be continued in the same direction.

Now, suppose no current to be flowing in the ring, then, on applying force to produce the same motion as before, the induced current will be in the opposite direction, *i. e.*, as the coil revolves towards the south pole and past it, the induced current in the coil is round the ring, as we look at it from behind, in the direction opposite to the motion of the hands of a watch.

With right-handed winding of the wire on the ring like a corkscrew, the current is coming towards the observer, or in the opposite direction to the motion of the ring on the side nearest the south pole.

If we consider a section of the ring as it approaches and goes away from the north pole of the magnet, the induced current in the coil, as the observer looks at it from behind, will be in the same direction as the hands of a watch move, so that the current will be away from the observer, or in the same direction as the motion of the ring on the side nearest the north pole. Hence, currents flow opposite ways round the ring, in the two halves of the ring, and meet at points equidistant from the two poles.

Dynamo-Electric Machines.

When the armature of a dynamo machine is turned, the amount of work which is produced by means of it is proportional to the number of turns of the armature per minute. If the same current passes through the electro-magnet and the armature, then the current in one acting on the current in the other will attract or repel it with a force proportional to the product of two currents, *i. e.*, to the square of the current The action between the currents is increased four-fold when the current in each is doubled. Hence, when such a machine is used as a generator of an electric current, the external work which can be done by that current in the electric arc or elsewhere is proportional to the square of the current.

Use of Separate Exciting Machine.

In all dynamo-electric machines, where the same current passes round the magnet and the armature, any disturbance in the resistance of the outer circuit, in the electric arc for instance, at once alters the current, and this alters the strength of the magnetic field, which again produces a further disturbance in the current, so that any disturbance is intensified, just as the permanent magnetism of the iron core is intensified by the action of the current in the machine on itself.

To obtain greater regularity, Wilde, in 1863, proposed to employ a separate continuous current machine to give a permanent magnetic field and to revolve the armature of the second machine between the poles of the magnet which is excited by the first machine. In order to find the yield or effective work of these machines, and their efficiency for electric lighting or for other purposes, we have seen, from the laws of Ohm and Joule, that measurements of current and of the work done by the current must be made.

In the last lecture I indicated four methods of making such measurements, viz.:

1. The galvanometer method.
2. The heat method, *i. e.*, by the change of temperature produced by the current in a wire of given resistance.
3. The electrometer or potentiometer method.
4. The electro-dynamometer method, *i. e.*, by the attraction between different parts of the same current.

Efficiency of Magnetic and Magneto-Electric Machines.

If we take a battery in a closed circuit, we know, from the laws of Faraday, that the amount of current produced is directly proportional to the weights of the chemical elements decomposed in each of the cells of the battery, the quantity of zinc dissolved in the battery being a measure of the quantity of current which has passed. According to the laws of transformation of energy, the work done in the chemical actions is here equivalent to the work done in heating the circuit. We may express the energy or the work done by electric currents in the same way as we express the energy of a head of water by the pressure multiplied by quantity of water. The electro-motive force corresponds to pressure, and the current flowing to the quantity of

water, so that the work done by the current is the product of electro-motive force by the quantity of electricity.

The work which can be done in the circuit is E_oC_o, where E_o is the electro-motive force of the battery, and this is spent in heating the resistance. Hence, $E_oC_o=C_o^2R$, where C_o is the current produced through a resistance R. Now, if any portion of the circuit, carrying a current, be set in motion, under the action of exterior magnetic forces, or under the influence of the mutual reactions of the currents in the fixed and movable parts, then the equivalent to the chemical actions in the battery will be spent in producing the heating of the conductor, and partly in doing the work of the electro-dynamic or magnetic forces. Representing the work done in producing motion by K, and the current in this case by C, we get $E_oC=C^2R+K$. The work K is equivalent to the external work done by the current C. Now, this external work K gives rise to an opposing electro-motive force in the induced circuit, and the energy of this induced current is $EC=K$; or $E=\frac{K}{C}$ is the electro-motive force due to induction.

The efficiency of an induction machine, when used as a motor, is the ratio of the work K to the total work $K+C^2R$, *i. e.*, the ratio of the electro-motive force of induction to the total electro-motive force of the battery. The efficiency $\rho=\frac{E}{E_o}=\frac{K}{K+C^2R}=\frac{1}{1+\frac{C^2R}{K}}$. The effective work $K=EC$ and $CR=E_o-E$. Therefore, $K=\frac{E.(E_o-E).}{R}$

If, then, a battery of electro-motive force E_o be employed, and an induction machine be employed as a motor, then the effective work depends on the product of two quantities, one of which increases as fast as the other diminishes.

Now, if one quantity increases as fast as another diminishes, their product is greatest when the two are equal. Hence such a machine is most effective when $E=E_o-E$, *i. e.*, when $E=\frac{1}{2}E^o$, so that $K=C^2R$ and $\rho=\frac{1}{2}$.

Half the work of the battery is then spent in heating the circuit and the other half in doing the external work. This corresponds to the case where the strength of the battery current is diminished by one-half through the effects of induction. If the same machine be

employed to produce a current of electricity by applying external work to it to turn it, then the energy of the induced current is equal to the work done by the currents during the motion, or $K=EC$. If K is greater than EC at first, then the machine will go faster and faster, and E and C will increase until the product becomes equal to K, when the motion will remain steady. Hence, such a machine should give induction currents of the greatest efficiency when used as a motor.

The above conclusions have been arrived at by considering the attractive and repulsive forces between different parts of a battery current, where the electro-motive force of the battery does not change.

Magnetic Machines.

The same reasoning will apply to the case where a permanent magnet is used in place of the original battery current, a closed circuit being moved in the magnetic field, or a magnet moved in the neighborhood of a fixed coil. In fact, the theory of Ampère requires that a small closed current should be equivalent to a magnetic molecule, and so a collection of equal small closed currents, all going the same way and occupying a given area, is equivalent to a collection of equal magnetic molecules with their poles in the same direction, *i. e.*, to a magnet. But a collection of equal small closed currents, all going the same way and occupying a given area, is equivalent to a current around that area.

We may readily express the efficiency of magneto-electric machines by a formula. Taking k to be the effective work done by a unit current in one turn of the armature, and n the number of turns per minute, and C the strength of the current, the total effective work is C^2nk, and the work done in overcoming the resistance, R, of the circuit is $R^2E\theta$; hence, by Joule's and Ohm's laws, if C be the electro-motive force, $EC=C^2nk+C^2R$.

Hence, the efficiency $= \frac{C^2nk}{C^2nk+C^2R} = \frac{1}{1+\frac{R}{nk}}$ Thus, for a given resistance, the greater the number of turns of a machine, the greater is the efficiency, and the higher the resistance, the less the efficiency. This formula shows that machines and lamps of high resistance can only be efficient when the machines are revolving at a high speed.

We may represent the efficiency of batteries or magnetic machines which are employed to drive an electro-motor, such as a dynamo-elec-

tric machine, by a diagram from *La Lumière Electrique*, in which the electro-motive force of the battery or exciting machine is measured on a horizontal line, and either the current or the energy expended is represented by vertical lines. The total electrical energy, represented by a straight line (III), is the sum of the effective work (IV), and the loss of energy by heating, etc. (VI).

It will be seen that when the efficiency is $\frac{1}{2}$, the greatest amount of effective work is produced, and this amount of work is one-half the total electrical energy. Since the electro-motive force is proportional to the number of revolutions a minute, this diagram gives the effective work and efficiency at different speeds.

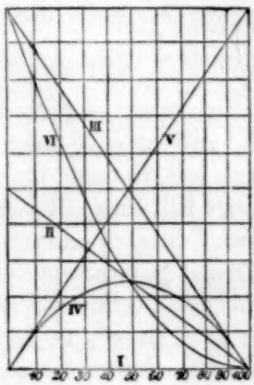
In the case where the fixed and the movable parts of the machine are electro-magnets, and the same current passes round both, then we have—

(1.) The action of the currents on one another.

(2.) The action of the fixed magnetic core on the movable coil, and of the movable magnetic core on the fixed coil.

(3.) The action of the two magnetic cores on one another. Hence, the value of k is of the form of $a+bm+cm^2$, where a, b, c are constants, and m depends on the magnetic properties of the cores.

FIG. 3.



I. Electro-motive force.
II. Strength of current.
III. Energy converted into electricity.
IV. Effective work in the external circuit.
V. The efficiency.
VI. Energy not converted into useful work.

Each of these actions arises from the influence of two equal currents on one another, and, therefore, will be proportional to the square of the current and to the number of turns of the coil in a minute, so that K is proportional to $nC^2(a+bm+cm^2)$ and the efficiency

$$\rho = \frac{1}{1+c_2R} = \frac{1}{1+\dfrac{R}{n(a+bm+cm^2)}}.$$

The most important part of the action in such machines depends on the action of the two magnetic cores one upon another. The reactions between the fixed and the movable cores produces great disturbance, in consequence of the cores not taking up their full magnetism

instantly, but requiring time for their full magnetization. In consequence of this, in all dynamo-electric machines, it is necessary to allow the rotation to go on, through an angle determined by the retardation of the magnetism of the cores, before taking away the current from the machine to do external work; hence, in order to get the greatest current, the springs for making contact must in such cases be shifted round in the direction in which the rotation is taking place. Faraday attributed this retardation to the time required to develop the molecular currents in the molecules of the magnet.

Alternate-Current Magneto Machines.

In alternate-current machines, there is no commutator for making the current continuous; but the currents from the coil are collected and sent through the external resistance in opposite directions for every half-turn of the armature. The earliest of these was the "Alliance" magneto-electric machine, which has been adopted by the French government for lighthouse illumination. Holmes converted this into a continuous current machine, and was the first to produce, in 1858, the electric light on a grand scale for lighthouse illumination. He afterwards removed the commutator and again converted it into an alternate-current machine.

The four methods of placing the coils on the revolving wheel, in machines for electric lighting, which have been employed in lighthouses, have been summed up by Mr. Douglass:

1. In Holmes' magneto-electric machine, the bobbins are arranged transversely, with their axes parallel to the axis of rotation around the circumference at the wheel.

2. In Siemens' machines, the wires are lengthwise and revolve about the long axis of the piece on which they are wound.

3. In Gramme's machines, the wires form a helix around a ring.

4. In the De Meritens machines, the wires are wound as in the Gramme, but are divided into separate parts which are insulated from one another and, passing in succession in front of opposite poles of magnets, give off alternate currents.

The Siemens Alternate-Current Machine.

A central disk carrying bobbins is set at right angles to a shaft and revolves between two sets of electro-magnets ranged in circles on each side of the disk, having their axes parallel to the shaft. The bobbins

have no iron cores, and so the heating caused by magnetization and demagnetization of the iron is avoided. The electro-magnets are excited by a small Siemens continuous-current machine.

This is similar to Wilde's dynamo-electric machine, which he produced in 1866, except that in the coils on his cast-iron disk Wilde placed soft iron cores and arranged them so as to form eight groups of four each. The current from one of these groups excites the electro-magnets, whilst the other seven groups give out the current for external use. Among the more recent alternate-current machines, arranged so as to be excited by a separate continuous-current machine, there is one which has lately been invented by M. Gramme, in which the bobbin of the continuous-current machine, which excites the magnets, is placed on the same axis with the rotating armature which gave the alternate currents, so that the two turn with the same velocity and the combined machines run at the same rate. This is much simpler than having two machines, and it is called a self-acting machine.

From principles which I have already explained, the greatest amount of effective work or yield obtained from an alternate-current machine, driven by a separate exciter, is not more than 50 per cent. of the electrical work given out by the first machine.

Result of MM. Mascart and Jamin's Experiments.

M. Jamin has found that, in the Alliance machine for electric light and giving alternate currents, the strength of current can be calculated by Ohm's law, considering the electro-motive force as proportional to the velocity, but replacing the resistance of the bobbins by a resistance about eight times as great. The resistance to be added is proportional to the velocity of the machine.

All theoretical determinations of the efficiency of machines are complicated by the retardation of magnetization of the magnets, which necessitates a change of position of the commutator in the direction of the rotation of the armature. If this change were not made, then it would be possible, with a bobbin in which the wire resistance was high, to get currents in opposite directions when the coil is rotated at different rates. For slow rotation, the galvanometer needle is deviated to one side; on increasing the velocity of rotation, the current is increased, at last reaches a maximum, and beyond that diminishes rapidly to nothing and becomes negative; as the velocity

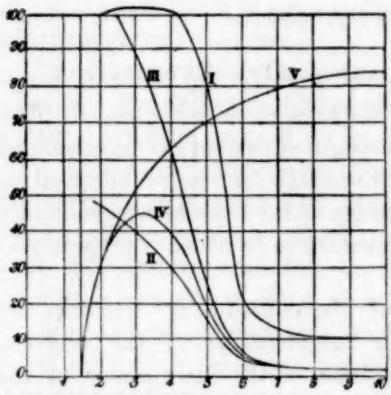
is still increased the current reaches its greatest negative value, and then increases again, and may have several such fluctuations.

The efficiency of the Siemens dynamo-electric machines has been examined experimentally by Dr. C. W. Siemens, who has communicated the results of his investigations to the Royal Society, and by Dr. Hopkinson, whose results are published in the *Transactions of the Institution of Mechanical Engineers;* also M. Hospitalier and Messrs. Auerbach and Meyer have experimented on Gramme machines, and MM. Mascart and Angot have considered the subject, both theoretically and practically, in some excellent papers which have been given in the *Journal de Physique.*

Taking the experiments of M. Hospitalier, as given in *La Lumière Electrique,* we get the results, as shown in the diagram:

	Ohms.
The total resistance of the machine before the experiment was .	1·185
" " of the heated bobbin	·75
" " of the heated electro-magnet, . .	·72
Total, ...	1·47

FIG. 4.



I. Electromotive force.
II. Strength of current.
III. Energy converted into electricity.
IV. Useful work in the external circuit.
V. Efficiency.

The resistances are laid down on the horizontal line to the scale of 1 c.m. per ohm.

The scale of electro-motive force (I) is 1 c.m. for 10 volts.

The scale for currents (II) is 1 c.m. for 10 webers.

The scale for work (III and IV) is 1 c.m. for 20 kilogrammetres.

Curve V represents the efficiency, *i. e.*, the ratio of the effective work in the outer circuit to the total work converted into electricity.

This curve expresses the ratio of the external work to the total work produced, and approaches the value unity as the resistance is increased; but for high resistances very little work is produced. From the curves it would appear that this Gramme machine does most effective work in the outer current,

without greatly heating the internal circuit, when the total resistance is about 4 ohms, *i. e.*, when the external resistance is about twice as great as the internal resistance. For less resistances the machine is greatly heated from the amount of internal work consumed, and for higher resistances the work converted into electricity becomes very small. The following numbers give the results of one experiment with the dynamo machine:

Number of turns a minute,	1000
External resistance,	2·7 ohms
Current,	25·5 webers
Electro-motive power,	107 volts
Total work converted into electricity,	3·64 h. p.
Effective work in external circuit,	2·38 h. p.

The efficiency in this case is 65 per cent. The effective work in the outer circuit is not more than 50 per cent. of the total work done to produce it.

(To be continued.)

Cometary Spectra.—Thollon has presented to the French Academy an account of some comparative observations upon the spectra of the two recent comets. In the first the continuous spectrum predominated, and finally it almost obscured the band spectrum. In the second the three carbon bands were very well defined and very brilliant, but gradually fading upon the side of the violet, and he was not able to observe any slight traces of a continuous spectrum until the night of the 21st of August. The brilliancy of the head and tail varies rapidly with the distance from the sun. In the absence of any precise photometric measurements the variations seemed to be nearly in the inverse ratio of the square of the distance. If this law should hold rigorously it would probably follow that the white light of comets is almost entirely due to reflection of the sunlight. It seems hard to reconcile the slowness with which the brilliancy of the band spectrum varies with the common opinion that the elements of a comet are raised to incandescence by the sun's heat. Comets, like nebulæ, may have some heat and light of their own, independent of that which may come from solar, mechanical and electric action.—*Comptes Rendus.* C.

On the SOURCES of ENERGY in NATURE AVAILABLE to MAN for the PRODUCTION of MECHANICAL EFFECT.

By Sir William Thomson.

Address delivered before the Physical Science Section of the British Association, 1881.

During the fifty years' life of the British Association, the Advancement of Science for which it has lived and worked so well has not been more marked in any department than in the one which belongs very decidedly to the Mathematical and Physical Section—the science of Energy. The very name energy, though first used in its present sense by Dr. Thomas Young about the beginning of this century, has only come into use practically after the doctrine which defines it had, during the first half of the British Association's life, been raised from a mere formula of mathematical dynamics to the position it now holds of a principle pervading all nature and guiding the investigator in every field of science.

A little article communicated to the Royal Society of Edinburgh, a short time before the commencement of the epoch of energy, under the title "On the Sources Available to Man for the Production of Mechanical Effect,"* contained the following:

"Men can obtain mechanical effect for their own purposes by working mechanically themselves and directing other animals to work for them, or by using natural heat, the gravitation of descending solid masses, the natural motions of water and air, and the heat, or galvanic currents, or other mechanical effects produced by chemical combination, but in no other way at present known. Hence the stores from which mechanical effect may be drawn by man belong to one or other of the following classes:

"I. The food of animals.

"II. Natural heat.

"III. Solid matter found in elevated positions.

"IV. The natural motions of water and air.

"V. Natural combustibles (as wood, coal, coal-gas, oils, marsh-gas, diamond, native sulphur, native metals, meteoric iron).

* Read at the Royal Society of Edinburgh on February 2d, 1852. (*Proceedings* of that date.)

"VI. Artificial combustibles (as smelted or electrically-deposited metals, hydrogen, phosphorus).

"In the present communication, known facts in natural history and physical science, with reference to the sources from which these stores have derived their mechanical energies, are adduced to establish the following general conclusions:

"*Heat radiated from the sun* (sunlight being included in the term) *is the principal source of mechanical effect available to man.** From it is derived the whole mechanical effect obtained by means of animals working, water-wheels worked by rivers, steam-engines, galvanic engines, windmills, and the sails of ships.

"2. The motions of the earth, moon, and sun, and their mutual attractions, constitute an important source of available mechanical effect. From them all, but chiefly no doubt from the earth's motion of rotation, is derived the mechanical effect of water-wheels driven by the tides.

"3. The other known sources of mechanical effect available to man are either terrestrial—that is, belonging to the earth, and available without the influence of any external body—or meteoric—that is, belonging to bodies deposited on the earth from external space. Terrestrial sources, including mountain quarries and mines, the heat of hot springs, and the combustion of native sulphur, perhaps also the combustion of inorganic native combustibles, are actually used; but the mechanical effect obtained from them is very inconsiderable, compared with that which is obtained from sources belonging to the two classes mentioned above. Meteoric sources, including only the heat of newly-fallen meteoric bodies, and the combustion of meteoric iron, need not be reckoned among those available to man for practical purposes."

Thus we may summarize the natural sources of energy as tides, food, fuel, wind and rain.

Among the practical sources of energy thus exhaustively enumerated, there is only one not derived from sun-heat—that is the tides. Consider it first. I have called it *practical*, because tide-mills exist. But the places where they can work usefully are very rare, and the whole amount of work actually done by them is a drop to the ocean of work done by other motors. A tide of two metres' rise and fall, if

* A general conclusion equivalent to this was published by Sir John Herschel, in 1833. See his *Astronomy*, edit. 1849, § (399).

we imagine it utilized to the utmost by means of ideal water-wheels doing with perfect economy the whole work of filling and emptying a dock-basin in infinitely short times at the moment of high and low water, would give just one metre-ton per square metre of area. This work done four times in the twenty-four hours amounts to $\frac{1}{1620}$ of the work of a horse-power. Parenthetically, in explanation, I may say the French metrical equivalent (to which in all scientific and practical measurements we are irresistibly drawn, notwithstanding a dense barrier of insular prejudice most detrimental to the islanders)—the French metrical equivalent of James Watt's "horse-power" of 550 foot-pounds per second, or 33,000 foot-pounds per minute, or nearly two million foot-pounds per hour, is 75 metre-kilogrammes per second, or $4\frac{1}{2}$ metre-tons per minute, or 270 metre-tons per hour. The French ton of 1000 kilogrammes used in this reckoning is 0·984 of the British ton.

Returning to the question of utilizing tidal energy, we find a dock area of 162,000 square metres (which is little more than 400 metres square) required for 100 horse-power. This, considering the vast costliness of dock construction, is obviously prohibitory of every scheme for economizing tidal energy by means of artificial dock-basins, however near to the ideal perfection might be the realized tide-mill, and however convenient and non-wasteful the accumulator — whether Faure's electric accumulator, or other accumulators of energy hitherto invented, or to be invented, which might be used to store up the energy yielded by the tide-mill during its short harvests about the times of high and low water, and to give it out when wanted at other times of six hours. There may, however, be a dozen places possible in the world where it could be advantageous to build a sea-wall across the mouth of a natural basin or estuary, and to utilize the tidal energy of filling it and emptying it by means of sluices and water-wheels. But if so much could be done, it would in many cases take only a little more to keep the water out altogether, and make fertile land of the whole basin. Thus we are led up to the interesting economical question, whether is forty acres (the British *agricultural* measure for the area of 162,000 square metres) or 100 horse-power more valuable. The annual cost of 100 horse-power night and day, for 365 days of the year, obtained through steam from coals, may be about ten times the rental of forty acres at £2 or £3 per acre. But the value of land is essentially much more than its rental, and the rental of land is apt to be much more

than £2 or £3 per acre in places where 100 horse-power could be taken with advantage from coal through steam. Thus the question remains unsolved, with the possibility that in one place the answer may be *one hundred horse-power*, and in another *forty acres*. But, indeed, the question is hardly worth answering, considering the rarity of the cases, if they exist at all, where embankments for the utilization of tidal energy are practicable.

Turning now to sources of energy derived from sun-heat, let us take the wind first. When we look at the register of British shipping and see 40,000 vessels, of which about 10,000 are steamers and 30,000 sailing ships, and when we think how vast an absolute amount of horse-power is developed by the engines of those steamers, and how considerable a proportion it forms of the whole horse-power taken from coal annually in the whole world at the present time, and when we consider the sailing ships of other nations, which must be reckoned in the account, and throw in the little item of windmills, we find that, even in the present days of steam ascendency, old-fashioned Wind still supplies a large part of all the energy used by man. But however much we may regret the time when Hood's young lady, visiting the fens of Lincolnshire at Christmas, and writing to her dearest friend in London (both sixty years old now if they are alive), describes the delight of sitting in a bower and looking over the wintry plain, not desolate, because "windmills lend revolving animation to the scene," we cannot shut our eyes to the fact of a lamentable decadence of wind-power. Is this decadence permanent, or may we hope that it is only temporary? The subterranean coal-stores of the world are becoming exhausted surely, and not slowly, and the price of coal is upward bound—upward bound on the whole, though no doubt it will have its ups and downs in the future as it has had in the past, and as must be the case in respect to every marketable commodity. When the coal is all burned; or, long before it is all burned, when there is so little of it left and the coal-mines from which that little is to be excavated are so distant and deep and hot that its price to the consumer is greatly higher than at present, it is most probable that wind-mills or wind-motors in some form will again be in the ascendant, and that wind will do man's mechanical work on land at least in proportion comparable to its present doing of work at sea.

Even now it is not utterly chimerical to think of wind superseding coal in some places for a very important part of its present duty—that

of giving light. Indeed, now that we have dynamos and Faure's accumulator, the little want to let the thing be done is cheap windmills. A Faure cell containing 20 kilogrammes of lead and minium, charged and employed to excite incandescent vacuum-lamps, has a light-giving capacity of 60-candle hours (I have found considerably more in experiments made by myself; but I take 60 as a safe estitimate). The charging may be done uninjuriously, and with good dynamical economy, in any time from six hours to twelve or more. The drawing off of the charge for use may be done safely, but somewhat wastefully, in two hours, and very economically in any time of from five hours to a week or more. Calms do not last often longer than three or four days at a time. Suppose then, that a five days' storage-capacity suffices (there may be a little steam-engine ready to set to work at any time after a four days' calm, or the user of the light may have a few candles or oil-lamps in reserve, and be satisfied with them when the wind fails for more than five days). One of the 20-kilogramme cells charged when the windmill works for five or six hours at any time, and left with its 60-candle hours' capacity to be used six hours a day for five days, gives a 2-candle light. Thus thirty-two such accumulator cells so used would give as much light as four burners of London 16-candle gas. The probable cost of dynamo and accumulator does not seem fatal to the plan, if the windmill could be had for something comparable with the prime cost of a steam-engine capable of working at the same horse-power as the windmill when in good action. But windmills as hitherto made are very costly machines; and it does not seem probable that, without inventions not yet made, wind can be economically used to give light in any considerable class of cases, or to put energy into store for work of other kinds.

Consider, lastly, rain-power. When it is to be had in places where power is wanted for mills and factories of any kind, water-power is thoroughly appreciated. From time immemorial, water-motors have been made in large variety for utilizing rain-power in the various conditions in which it is presented, whether in rapidly-flowing rivers, in natural waterfalls, or stored at heights in natural lakes or artificial reservoirs. Improvements and fresh inventions of machines of this class still go on; and some of the finest principles of mathematical hydrodynamics have, in the lifetime of the British Association, and, to a considerable degree, with its assistance, been put in requisition for

perfecting the theory of hydraulic mechanism and extending its practical applications.

A first question occurs: Are we necessarily limited to such natural sources of water-power as are supplied by rain falling on hill-country, or may we look to the collection of rain-water in tanks placed artificially at sufficient heights over flat country to supply motive-power economically by driving water-wheels? To answer it: Suppose a height of 100 metres, which is very large for any practicable building, or for columns erected to support tanks; and suppose the annual rainfall to be three quarters of a metre (30 inches). The annual yield of energy would be 75 metre-tons per square metre of the tank. Now one horse-power for 365 times 24 hours is 236,500 foot-tons; and therefore (dividing this by 75) we find 3153 square metres as the area of our supposed tank required for a continuous supply of one horse-power. The prime cost of such a structure, not to speak of the value of the land which it would cover, is utterly prohibitory of any such plan for utilizing the motive power of rain. We may or may not look forward hopefully to the time when windmills will again "lend revolving animation" to a dull flat country; but we certainly need not be afraid that the scene will be marred by forests of iron columns taking the place of natural trees, and gigantic tanks overshadowing the fields and blackening the horizon.

To use rain-power economically on any considerable scale we must look to the natural drainage of hill country, and take the water where we find it either actually falling or stored up and ready to fall when a short artificial channel or pipe can be provided for it at moderate cost. The expense of aqueducts, or of underground water-pipes, to carry water to any great distance—any distance of more than a few miles or a few hundred yards—is much too great for economy when the yield to be provided for is *power;* and such works can only be undertaken when the *water itself* is what is wanted. Incidentally, in connection with the water supply of towns, some part of the energy due to the head at which it is supplied may be used for power. There are, however, but few cases (I know of none except Greenock) in which the energy to spare over and above that devoted to bringing the water to where it is wanted, and causing it to flow fast enough for convenience at every opened tap in every house or factory, is enough to make it worth while to make arrangements for letting the water-power be used without wasting the water-substance. The cases in which water-

power is taken from a town supply are generally very small, such as working the bellows of an organ or "hair brushing by machinery," and involve simply throwing away the used water. The cost of energy thus obtained must be something enormous in proportion to the actual quantity of the energy, and it is only the smallness of the quantity that allows the convenience of having it when wanted at any moment, to be so dearly bought.

For anything of great work by rain-power, the water-wheels must be in the place where the water supply with natural fall is found. Such places are generally far from great towns, and the time is not yet come when great towns grow by natural selection beside waterfalls for power, as they grow beside navigable rivers for shipping. Thus hitherto the use of water-power has been confined chiefly to isolated factories which can be conveniently placed and economically worked in the neighborhood of natural waterfalls. But the splendid suggestion made about three years ago by M. Siemens in his presidential address to the Institution of Mechanical Engineers, that the power of Niagara might be utilized by transmitting it electrically to great distances, has given quite a fresh departure for design in respect to economy of rain-power. From the time of Joule's experimental electro-magnetic engines, developing 90 per cent. of the energy of a voltaic battery in the form of weights raised; and the theory of the electro-magnetic transmission of energy completed thirty years ago on the foundation afforded by the train of experimental and theoretical investigations, by which he established his dynamical equivalent of heat in mechanical, electric, electro-chemical, chemical, electro-magnetic and thermo-electric phenomena, it had been known that potential energy from any available source can be transmitted electro-magnetically by means of an electric current through a wire, and directed to raise weights at a distance, with unlimitedly perfect economy. The first large-scale practical application of electro-magnetic machines was proposed by Holmes in 1854, to produce the electric light for lighthouses, and persevered in by him till he proved the availability of his machine to the satisfaction of the Trinity House and the delight of Faraday, in trials at Blackwall in April, 1857, and it was applied to light the South Foreland lighthouse on December 8, 1858. This gave the impulse to invention; by which the electro-magnetic machine has been brought from the physical laboratory into the province of engineering, and has sent

back to the realm of pure science a beautiful discovery—that of the fundamental principle of the dynamo, made triply and independently, and as nearly as may be simultaneously, in 1867, by Dr. Werner Siemens, Mr. S. A. Varley and Sir Charles Wheatstone; a discovery which constitutes an electro-magnetic analogue to the fundamental electrostatic principle of Nicholson's revolving doubler, resuscitated by Mr. C. F. Varley in his instrument "for generating electricity," patented in 1860, and by Holtz in his celebrated electric machine, and by myself in my "replenisher" for multiplying and maintaining charges in Leyden jars for heterostatic electrometers, and in the electrifier for the siphon of my recorder for submarine cables.

The dynamos of Gramme and Siemens, invented and made in the course of these fourteen years since the discovery of the fundamental principle, give now a ready means of realizing economically on a large scale, for many important practical applications, the old thermo-dynamics of Joule in electro-magnetism; and, what particularly concerns us now in connection with my present subject, they make it possible to transmit electro-magnetically the work of waterfalls through long insulated conducting wires and use it at distances of fifties or hundreds of miles from the source, with excellent economy —better economy, indeed, in respect to proportion of energy used to energy dissipated, than almost anything known in ordinary mechanics and hydraulics, for distances of hundreds of yards instead of hundreds of miles.

In answer to questions put to me in May, 1879,* by the Parliamentary Committe on Electric Lighting, I gave a formula for calculating the amount of energy transmitted and the amount dissipated by being converted into heat on the way, through an insulated copper conductor of any length, with any given electro-motive force applied to produce the current. Taking Niagara as example, and with the idea of bringing its energy usefully to Montreal, Boston, New York and Philadelphia, I calculated the formula for a distance of 300 British statute miles (which is greater than the distance of any of those four cities from Niagara, and is the radius of a circle covering a large and very important part of the United States and British North America), I found almost to my surprise that, even with so great a distance to be provided for, the conditions are thoroughly practicable

* Printed in the Parliamentary Blue Book Report of the Committee on Electric Lighting, 1879.

with good economy, all aspects of the case carefully considered. The formula itself will be the subject of a technical communication to Section A in the course of the meeting on which we are now entering. I therefore, at present, restrict myself to a slight statement of results:

1. Apply dynamos driven by Niagara to produce a difference of potential of 80,000 volts between a good earth-connection and the near end of a solid copper wire of half an inch (1·27 centimetres) diameter and 300 statute miles (483 kilometres) length.

2. Let resistance by driven dynamos doing work, or by electric lights, or—as I can now say—by a Faure battery taking in a charge, be applied to keep the remote end at a potential differing by 64,000 volts from a good earth-plate there.

3. The result will be a current of 240 webers through the wire taking energy from the Niagara end at the rate of 26,250 horse-power, losing 5250 (or 20 per cent.) of this by the generation and dissipation of heat through the conductor, and 21,000 horse-power (or 80 per cent. of the whole) on the recipients at the far end.

4. The elevation of temperature above the surrounding atmosphere, to allow the heat generated in it to escape by radiation and be carried away by convection is only about 20°Centigrade; the wire being hung freely exposed to air like an ordinary telegraph wire supported on posts.

5. The striking distance between flat metallic surfaces with difference of potentials of 80,000 volts (or 75,000 Daniells) is (Thomson's "Electrostatics and Magnetism," sec. 340) only 18 millimetres, and therefore there is no difficulty about the insulation.

6. The cost of the copper wire, reckoned at 8d. per lb., is £37,000; the interest on which at 5 per cent. is £1900 a year. If 5250 horse-power at the Niagara end costs more than £1900 a year, it would be better economy to put more copper into the conductor; if less, less. I say no more on this point at present, as the economy of copper for electric conduction will be the subject of a special communication to the Section.

I shall only say, in conclusion, that one great difficulty in the way of economizing the electrical transmitting power to great distances (or even to moderate distances of a few kilometres) is now overcome by Faure's splendid invention. High potential—as Siemens, I believe, first pointed out—is the essential for good dynamical economy in the electric transmission of power. But what are we to do

with 80,000 volts when we have them at the civilized end of the wire? Imagine a domestic servant going to dust an electric lamp with 80,000 volts on one of its metals! Nothing above 200 volts ought on any account ever to be admitted into a house or ship, or other place where safeguards against accident cannot be made absolutely and forever trustworthy against all possibility of accident. In an electric workshop, 80,000 volts is no more dangerous than a circular saw. Till I learned Faure's invention I could but think of step-down dynamos, at a main receiving station, to take energy direct from the electric main with its 80,000 volts, and supply it by secondary 200-volt dynamos or 100-volt dynamos, through proper distributing wires, to the houses and factories and shops where it is to be used for electric lighting and sewing machines and lathes and lifts, or whatever other mechanism wants driving power. Now the thing is to be done much more economically, I hope, and certainly with much greater simplicity and regularity, by keeping a Faure battery of 40,000 cells always being charged direct from the electric main, and applying a methodical system of removing sets of 50 and placing them on the town-supply circuits, while other sets of 50 are being regularly introduced into the great battery that is being charged, so as to keep its number always within 50 of the proper number, which would be about 40,000 if the potential at the emitting end of the main is 80,000 volts.

Malleable Iron.—M. Forquignon has published an extensive series of researches upon malleable iron and the re-heating of steel. Among other conclusions he attaches special importance to the following: 1. Malleable iron always contains amorphous graphite; 2. A casting may lose carbon and yet remain brittle if the original quantity of graphite is not increased; 3. A casting may become malleable without losing any sensible portion of its carbon; 4. If silicium is added to manganesian castings they are improved by re-heating; 5. Hydrogen and nitrogen may unite with the carbon of a casting so as to make it malleable without the production of graphite; 6. The breaking load is always more than doubled, sometime more than quadrupled, by annealing. It increases with the duration of the heating, very rapidly at first and then very slowly; 7. Ductility generally increases with the resistance to breaking, but after a certain limit it has a slight tendency to diminish.—*Ann. de Chim. et de Phys.* C.

RADIO-DYNAMICS: ANOTHER VERIFICATION OF PREDICTION.

By Pliny Earle Chase, LL.D.

On the 4th of October, 1878, I presented a communication to the American Philosophical Society,* in which I showed that the position of Watson's first intra-mercurial planet, as computed by Gaillot and Mouchez, represented the third intra-mercurial term of my harmonic series. At the last meeting of the British Association, Prof. Balfour Stewart read a paper in which he gave indications of sun spot disturbances by a planet revolving in 24·011 days and consequently having a semi-axis major of ·163. This confirmation, both of my own prediction† and of the calculations of the French astronomers, is the more interesting because the first confirmation of my series was contained in a communication which was made to the Royal Society, by Messrs. De la Rue, Stewart and Lowry; forty-one days after I had announced the series to the Philosophical Society and published it in the *New York Tribune.*‡ The accordances are as follows:

1st interior harmonic term,	·267	De la Rue, S. and L.,	·267
3d " " "	·165	Gaillot and Mouchez,	·164
		Stewart,	·163

New Experiments in Harmonic Vibration. — Decharme mixes a little finely powdered minium in water and covers a horizontal plate of glass with it as uniformly as possible. He then lets fall upon this thin layer a drop of the mixture, and there appears a regular figure formed of the minium, arranged in rays and concentric rings, the whole producing very various designs. The figures resemble in many respects the vibratory forms of circular plates, which he has been studying. The three systems of the Chladni plates, the diametral, circular and compound, are usually coexisting, but either one can be made to predominate over the others at the will of the experimenter.—*Comptes Redus.* C.

* Proc. Am. Phil. Soc., xviii, 34–6.

† *Ibid.*, xiii, 238.

‡ *Ibid.*, p. 470.

Radiant Matter.—Father Serpieri and Prof. Riglio have published, in the *Revista Scientifico-Industriale*, some of their observations upon Crookes' apparatus. Ferrini has repeated the experiments and published the following theory: On account of the very great resistance which, as Hittorf has shown, is produced under the influence of a great rarefaction around the cathode, there arises a powerful induction, and the phosphorescence of the walls of the globe indicates their intersection with the lines of force of the electric field, which is most neatly circumscribed where those lines are the most energetic. The gaseous molecules arrange themselves along those lines while transmitting the electricity. The blue pencil, which corresponds to the most intense portion of the electric field, represents a continuous discharge between the cathode and the glass which is in front of it.—*La Lumière Electrique.* C.

Phosphorescent Alumina.—Crookes has experimented with phosphorescent spectra in a nearly perfect vacuum. After operating for a long time upon chemically pure alumina, which had been precipitated from the sulphate, he noticed a curious phenomenon. When the alumina was first enclosed in the vacuum it was of a snowy whiteness, but after having been exposed frequently to the molecular current, which made it phosphorescent, it gradually assumed a rosy tint, and after two years, when it was exposed to the solar light, it showed a trace of the aluminium line. Repeated molecular excitement gradually brings the amorphous powder into a crystalline form. If ammonia is added in great excess to a diluted solution of alum a part of the alumina is precipitated. If the solution is then filtered and boiled, the alumina which was dissolved by the ammoniacal excess is precipitated. Separating it by filtration, igniting it, and submitting it to the action of the molecular current, instead of giving a red light it has a pale green phosphorescence which, when examined with the prism, shows no lines, but simply a concentration of light in the green portion of the spectrum. In studying rubies, of which he examined an enormous number by his apparatus, Crookes had the good fortune to find a single crystal, which, although it presented no difference to the sight, gave a green light under the action of the molecular current. This green light, however, always had a trace of the red line, and if the action of the molecular current was continued for several minutes the green phosphorescence faded and a red tint was produced. —*Ann. de Chim. et de Phys.* C.

Radiant Absorption of Carbonic Acid. — By experiments upon the absorption of radiant heat by gases, E. Lechler finds that a layer of carbonic acid 917 millimetres (37 in.) thick absorbs 90 per cent. of the luminous radiation. The carbonic acid of our atmosphere is therefore sufficient fully to account for the atmospheric absorption of the Sun's rays.—*Ann. d. Physik.* C.

Illumination of Tubes by the Holtz Machine.—The Geissler tubes can be illuminated by the Holtz machine, but the light is weak; in order to give it brilliancy, condensers and frequent interruptions of the current are required; if stratifications are desired it is necessary to interpose resisting media. Most of the experiments with the Crookes tubes can be performed with the Holtz as well as with the induction coil. Some of the experiments, however, which require very strong coils, are not very satisfactory: such, for example, as the luminous cross, the repulsion of two rays of radiant matter, and the incandescence of platinum.—*Les Mondes.* C.

Constitution of Comets. — M. Prazmowski concludes, from observations since 1858 by the spectroscope and polariscope, that comets are formed of a condensed portion, which constitutes the nucleus, surrounded by an incandescent gaseous atmosphere, which contains carbon and reflects the solar light, and of a swarm of disaggregated material which is not controlled by the cometary attraction but moves in obedience to universal attraction. In some comets the polarization of the light is strongly marked, while in others it is almost wholly absent. In the latter case he compares the structure to that of atmospheric clouds.—*Comptes Rendus.* C.

Connection between Refraction and Absorption of Light. —Ketteler has conducted an extensive series of experiments in order to prove his hypothesis that the refraction and absorption of light are due to the same physical laws. In order to strengthen his results, he tested each both by spectral, metric and photometric methods, and thus was able to show a very satisfactory accordance between the refraction and absorption curves. Each class of constants was found to exhibit the same relations to the concentration of the liquids which were experimented upon, and he regards the experiments as conclusively establishing the identity which is assumed in his hypothesis.—*Wiedemann's Annalen.* C.

Book Notices.

WORKING DRAWINGS AND HOW TO MAKE AND USE THEM, designed for Industrial, Technical, etc., Schools and Artisans desiring a knowledge of the principles of Pattern and Template making. By Lewis M. Haupt, Professor of Civil Engineering in the University of Pennsylvania, etc. 12mo. Philadelphia: Jos. M. Stoddard & Co. 1881.

MR. EDITOR:—Having been absent from the city, I have only just received the August number of the Journal, which contains a review of my recent little book on "Working Drawings," etc., and as your author has manifestly misunderstood my preface or read it hastily, I desire to correct some of the erroneous impressions his review must create, to the prejudice of the work.

He states first generally that "Engineers and master mechanics * * have been amazed that even graduates of technical and scientific schools *generally* come to them with *entirely erroneous methods of thought** and practice."

If this statement be true, then the system of instruction in our scientific and technical schools generally must be out of gear, from the use of present standard authorities, not the book under review, as that has not yet been introduced, and it is time the schools should know it and change their system; but I have heard no complaint in that direction, and our graduates seem to have no difficulty in making drawings that are readily understood. At all events, no school should teach errors, and if our system be such we hope to be convinced of it that a change may be immediately effected.

In the second paragraph your reviewer states that "The title and preface of this book give the impression that the author has done a good work in presenting a plan by which cadet engineers can be graduated capable of entering the draughting room and being immediately useful," etc.

If such an impression is created by a careful reading of the preface I must confess that I need to study how to write correctly, for I have distinctly stated therein that the book was designed especially for the very lowest grade of scholars capable of comprehending the subject, viz., those in the public schools, and that this book was but the *first* of a *series* and was intended simply as a test of the ability of the

* The italics are my own.

student to comprehend the principles which underlie the *science* of making drawings. Yet notwithstanding this, your reviewer expects to find in this little volume of 55 pages, sold for 60 cents, and the first one of a series, sufficient instruction to give a draughtsman a "technical knowledge of scales, pencils, instruments, inks and colors, manual skill to use them with neatness and dispatch; and with some experience of the best American practice in regard to the making of working drawings, such as the relative arrangement of different views, the avoiding of unnecessary repetitions, the judicious use of sections, coloring and shade lines, and particularly with a knowledge of the best distribution of dimension lines and figures and with a neat and clear style of making them. An examination of the text will fail to confirm this impression. Instead of being the connecting link between theory and practice, it is based entirely on the theory of Descriptive Geometry and is in direct opposition to the practice of American and English engineers."

Concerning these objections I have only to say, 1st, that it is a waste of printer's ink to attempt to describe instruments and the manner of using them in books when the same information has already been repeated so frequently and when a few hours of practical instruction by a competent draughtsman would be worth more than any amount of description; 2d, that it seems unreasonable to expect the whole of a serial work to appear in its first volume and to condemn the remainder unseen; 3d, that I cannot conceive of a working drawing that is *not* "based *entirely* on the theory of Descriptive Geometry" or projections, intersections and developments; and, 4th, that the last objection is only partially true, since but a small percentage of "American and English engineers" use *any other* than the system of projections represented in the work in question.

Your reviewer uses the word engineer in a restricted sense, evidently meaning only mechanical engineers, and forgets that civil engineers, architects and mining engineers, as well as *many mechanical* engineers use precisely the methods given in this work. Also by far the greater number of foreign countries teach and use the same method, and in conforming to it I have simply followed the precedents of the great bulk of instructors and constructors in this and other countries.

Your reviewer's next paragraph conveys the idea that I have set up a claim for originality in introducing the theory of projections as

applied to working drawings, and quotes Davies' old work as a precedent. Now, as to this work, we instructors think it verbose and wearisome, and prefer the more concise, logical and clearer demonstrations of the late distinguished Prof. Church as tending, more than any other method, to develop intellectual conceptions—and this is the method I have followed in this little book. I make no such claim, but simply the adaptation of the subject to public school instruction.

As a man of science becomes skilled in his profession, it is only natural that he should *apparently* ignore first principles and soar higher with great ease, but as to the "best engineers having long since discarded these theories and methods, with their diedral angles, ground lines, projections, traces, etc.," I must beg leave to doubt. The *principles* are there, and must remain in use in every case just as much as the foundations of a house must remain under it, although unseen. Because these principles are exceedingly simple to an expert it does not follow that they do not need elaboration for the beginners, to whom they are entirely new. To them they must be presented in every phase, so that they may be well grounded in principles, as every teacher can testify.

Your reviewer states again that "A subject so simple as the making of a working drawing * * should not be put in the form of problem, theorem, analysis and construction, but should be based on the experience of eminent engineers," etc. But upon what is the experience of an engineer based, if not upon just such a logical course of reasoning? Must he not know, first, that he desires to create something, to accomplish a certain end? This is his *problem or theorem.* Must he not then *analyze* the machine or building, studying carefully the relations, proportions and operations of its several parts? This is the analysis. And, finally, after his conception is clear on the subject, and he has resolved it out in his brain, then, and only then, can he proceed to represent and *construct* it. And this is his construction. This logical proceeding must occur in every well-disciplined mind; and it is the function of the teacher to develop it as rapidly as possible. Hence the use of mathematics, and the reason why good mathematicians are generally excellent logicians and successful engineers.

So far as I can discover from the review before me, the whole weight of the objection to the book under consideration consists in the fact that whilst a very large number of draughtsmen recognize and use the method of *projecting* the face to be drawn upon a plane placed behind

it, a few prefer to make the drawing upon a transparent plane held in front of it. This is the sole difference; and because your critic is accustomed to the latter method he says that "a book, like the one under review, which teaches obsolete methods and incorrect technicalities, must certainly be misleading and injurious to the people whom it was intended to benefit."

As this is a broad and public assertion, apparently sanctioned by so high an authority as the Franklin Institute, I desire to know for the benefit of the future mechanics and engineers who may receive instruction from technical and scientific schools, if there is a *standard* of modern methods in existence where it is accessible to modern instructors, and, if not, I respectfully suggest that it would be eminently proper for your Committee on Science and the Arts to see that this important defect be supplied, and to recommend the abolition of such works as Church's "Descriptive Geometry," Warren's "Projection and Machine Drawing," Mahan's "Industrial Drawing," Binn's "Orthographic Projections," Watson's "Descriptive Geometry," and many others from the scientific schools of the country as tending to teach *errors*, and to mislead and confuse students, and to cause to be prepared a properly recognized and authenticated standard for use by engineers of every description, whether civil, mechanical, mining, architectural or topographical.

Very respectfully yours,

LEWIS M. HAUPT.

ELEMENTARY PROJECTION DRAWING, THEORY AND PRACTICE. By S. Edward Warren, C.E. Fifth edition. Revised, and with a new division on the Elements of Machines. 8vo. New York: John Wiley & Sons. 1880.

If this well-known book were properly revised and printed, so that it would be *studyable*, it would be highly useful. The greater portion of it is printed from old and worn-out plates, and the folding sheets of illustrations are not only extremely unsuitable for reference, but in many cases absolutely indecipherable. A work so filled with "primes" and "seconds" should be at least well printed.

The first division is on Elementary Projections, and, while very thorough, the treatment is as complicated and artificial as the lettering is illegible.

The second section (details of Masonry, Wood and Metal Construc-

tions) is very scant in its treatment of the various forms of timber joints. Thus, we would expect to find the various single, double, open, full and blind mortise and tenon joints clearly laid out, and blind and false doweling, and dovetailed and keyed miters shown. Piston rod packing is scantily treated, and from an ancient point of view.

In the third division (Elementary Shadows and Shading) lies the real merit of the book.

The fourth, on Isometrical and Oblique Projections, is well handled, the chapter on Oblique or Pictorial Projection being specially interesting.

The new fifth division, on Elements of Machines, is disappointing, especially in relation to gearing. For instance, the instructions, page 132, for construction of spur wheels, would be very obscure even if Plate XVII, to which they refer, were decipherable.

The sixth division, on Simple Structures and Machines, is very fair as a review of practice.

Altogether, we cannot help wishing that an author of such well-known ability as Prof. Warren had carefully revised the text, so as to put it within the comprehension of those not his equals in such matters, and that the publisher had been more liberal, not to say exacting, in the matter of illustrations. R. G.

THE FIGURE OF THE EARTH. An Introduction to Geodesy. By Mansfield Merriman, Professor of Civil Engineering in Lehigh University. 12mo. John Wiley & Sons, New York.

This little book of 88 pages has no other pretension than that of being an introduction to a course of study in geodesy, its substance being largely found in some familiar talks on "the size and shape of the earth," delivered before the students of civil engineering at the Lehigh University. Mr. Merriman calls attention to the fact that the science of geodesy would never have existed had we continued in the ancient paths. The history is given of the early attempts for the determining of the size of the earth from the time of Anaximander (—570) down to the present date, as the earliest measurements were made in the *stadiæ*, the size of which not being known, the comparisons between the ancient and the modern results cannot be made. The earth has been considered as a spheroid, ellipsoid, ovaloid and as a geoid, and arguments, *pro* and *con*, for these shapes, as well as the general manner of determining important properties of the same, have at various times occupied the consideration of savants. L. S. W.

Franklin Institute.

HALL OF THE INSTITUTE, Oct. 19th, 1881.

The stated meeting was called to order at 8 o'clock P.M., the President, Mr. William P. Tatham, in the chair.

There were present 99 members and 32 visitors.

The minutes of the last meeting were read and approved.

The Actuary presented the minutes of the Board of Managers, and announced that at the last meeting of the Board 21 persons were elected members of the Institute; also that a vacancy existed in the Board, caused by the resignation of Mr. Chabot.

Mr. Graff moved that the regular order of business be suspended, and that we proceed to make nominations, which was carried.

Mr. Graff nominated Mr. W. H. Thorne. There being no other nominations, the Secretary, upon motion of Mr. McKean, was directed to cast the Institute ballot for the gentleman above named, and the President thereupon declared him elected for the unexpired term of Mr. Chabot.

Mr. Lloyd Wiegand described William Nelson Barrow's improvement in moulding balls, shot and shell, the machine and some of its products being exhibited. The purpose of the invention is to produce articles of perfectly circular form and of exact dimensions by casting, and without recourse to grinding or any other finishing process. Two objects are to be served—one to reduce the cost of production, the other to leave the product with its hard outer skin, thus making it more durable. The castings exhibited were said to be accurate within one one-hundredth part of an inch and were balls (used without grinding or dressing) to draw lap-welded tubes over. The causes of imperfect castings are mainly in the imperfection of the sand mould in form and the rapping or jarring of the pattern to disengage it from the sand which renders the mould perceptibly larger. Barrow's moulding machine dispenses with the rapping, the pattern being withdrawn from the sand by mechanism which guides it accurately in its motion from the sand and holds the latter by a plate fitting around the pattern so as to prevent displacement. To avoid the imperfections due to the ordinary method of venting, the plates in Mr. Barrow's machine have

channels or chambers into which the rammer cannot reach. Venting needles pass through these channels, and are withdrawn with the pattern, leaving the sand loose in the channels, though closely compacted around the pattern. The core rests entirely in the lower part or bottom of the mould, is centered therein by its form, and constitutes the cover or cope of the mould, so that the possibility of maladjustment is avoided. The machine as described has been in use for more than a year, and the exhibited castings were taken indiscriminately from stock. The invention can, of course, be applied to the production of projectiles for ordnance.

Johnston's Air Compressor was shown in operation. The object of this invention, the Secretary stated, is to enable air to be compressed in an economical manner, and with less friction and loss of power than has heretofore been done. It consists of a fixed shaft, with a cylindrical casing, inclosed at its ends and journaled upon the shaft, provided within its upper portion with valve chambers, that contain inlet and outlet valves, and extend downward to the upper side of the shaft. Also a fixed partition, which extends between the lower side of the partition and the lower wall of the casing. Water fills the lower half of the casing, which is then caused to oscillate upon the shaft, the movement in each direction being continued until the front walls of the valve chamber are nearly horizontal, and impinge upon the surface of the water.

The front inlet valve in the direction of the movement will be closed, and the outlet valve of the same side opened, as the valve chambers approach the water-line, and air contained between the former and the surface of the water will be forced into the outlet chamber and into the discharge pipe, the pressure being governed by the relative quantity permitted to escape.

While the air is expelled from one side of the casing it is admitted to the opposite side through the inlet chamber, the alternate filling and discharging being caused by the oscillation of the casing.

In consequence of the practical incompressibility of the water, it presents a solid bearing against which the air is compressed by the downward movement of the valve chambers, which perform in this respect the office of a solid piston. The office of the partition is to hold the water stationary and prevent it from oscillating, as would otherwise be the case.

When a high pressure is desired two or more compressors may be placed upon the same axis, the second one taking the compressed air from the first, etc., the degree thus obtained being governed by the number used.

To keep down the temperature of the compressed air a small stream of water may be admitted with the air supply, the surplus being passed out with the air discharge, and afterwards separated therefrom in a receiver.

The casing can be driven by means of a connecting rod, journaled at one end upon a crank-pin, from the head of the casing, while the other end of the connecting rod is journaled upon the crank of the engine, although any other way of giving an oscillating motion may be adopted.

The mechanism can also be used as a pump, or for exhausting gas or air from mines, etc. When used as a pump the cylinder is submerged, or if above the water a suction pipe is connected.

Mr. Johnston, having been called upon, gave a further explanation of his invention. In reply to inquiries of Mr. Nystrom, he said that the water did not get hot, and that no packing was needed, as the rapid oscillations gave no time for the water to pass the loose fitting partition.

Mr. Wiegand said that he had found water packing used to seal pistons on compound pumps, for compressing nitrous oxide, and for compressing gas used in cars of the Pennsylvania Railroad, very efficient.

These pumps are similar to Mr. Johnston's compressor, in that the cylinders move and the pistons are stationary, leaving the water undisturbed. All the packings are continually covered with water, and the pressure for the compression of nitrous oxide reaches as high as 860 to 900 pounds to the square inch, with no injurious heating. Johnston's Universal Shaft Coupler was also exhibited.

Bennor's Automatic Seal Trap, which was shown, has a mercury seal joint and induction and eduction pipes, the latter enlarged so as to hold a considerable body of water. About a pound and a half of mercury is used in the trap. When the mercury is overbalanced by the water it spreads on an inclined platform forming the bottom of the chamber, thus allowing the water and matter flowing from the basin or sink to pass freely through the exit pipe, after which the

mercury returns to its former position and re-establishes the absolute seal. The high specific gravity of the mercury is relied upon to prevent any back pressure of gas from overcoming the seal and also to prevent syphonage and the evaporation that takes place in an ordinary water seal trap when not in use during the summer months.

Kemble's Lubricator for Wheel Hubs, also shown, has an oil-cup made in the form of a threaded tube, holding a supply of oil and intended to be screwed into the carriage-box, thus bringing the oil in contact with the axles.

Dr. Norris exhibited a new spectrum tube sent to him by Queen & Co., so arranged as to permit the observer to look at the spectrum of a gas from end to end, thus increasing the intensity of the light over that shown by the ordinary tubes, which are placed vertically and looked at transversely. The Secretary stated that in a difficult carbon spectrum, 31 lines were shown by the new tubes, all bright, while with the old forms only eight could be distinguished, and it was calculated a hundred times more light, nearly, was obtained.

Several other inventions were exhibited, among them being the following: H. J. Sills' Blotter, a flexible metal pad, with a convenient means of changing the slips of blotting-paper; Archer's chair for the use of physicians, which can be easily changed from one of ordinary height to a reclining lounge or put in any intermediate position; and the Auburndale metallic thermometers which, in several weeks' test at the Institute, kept within half a degree of a standard thermometer through a range in temperature of from thirty to forty degrees; also a section of the conduits used on Market street by the National Underground Electric Company. The necessary trench to be dug is about 4 feet deep by 18 inches wide and the bottom and sides to be used are lined with hydraulic cement. The tubes are made of tinned iron about two inches in diameter and are in convenient lengths, regulated by the size of the sheets of tin soldered together. Bands of tarred paper are wrapped around the tubes to prevent them from touching and to allow the spaces between them to be filled with the insulating material, which is a composition of asphaltum and slag. The tubes are twenty in number—laid in four rows of five each, one above the other—and each tube will hold a number of insulated wires. The work is completed with concrete on the top, filling in of earth, paving, etc. Manholes are constructed at each square for any necessary repairs.

Mr. Robert Grimshaw read a paper on the application of frictional

electricity to the purification of middlings, illustrated by drawings of the "electric purifier" thrown upon the screen. He first described briefly the old processes of milling, in which the middlings, although among the most valuable products, could not be utilized as flour because they could not be cleaned of bran and other impurities, and then described modern milling, in which the object is to get as many middlings as possible. These middlings have to be purified, and for this purpose various kinds of wind separators have been employed and recently the electrical purifier, of which drawings were exhibited. In these machines frictional electricity is used—hard rubber rolls, electrically excited, attracting to their surfaces the fine bran and lighter impurities from the middlings as the latter pass under them. The bran, etc., is then swept from the rolls by cushions, while the purified middlings are graded by passing through the sieves on which they are carried beneath the rolls. One great advantage of the electric over the wind purifiers is that they get rid of the danger of dust explosions, always present in mills that use currents of air for lifting the lighter impurities from the middlings. This apparatus has been in operation in the Atlantic Mills, Brooklyn, for a year, and the proprietors say that it saves them from ten to twenty cents on every barrel of flour.

Mr. Wm. V. McKean offered the following resolution, which was adopted:

"WHEREAS, attention has been called by the Randolph Street Mill fire to the subject of adequate fire escapes on tall buildings, and to real or supposed dangers attending the lighting the mills by electricity; and, whereas, it is desirable that mill-owners should be informed of the best means of preventing such fires and of affording means of escape for their operatives;

"*Resolved*, That the President be authorized to appoint two committees, one to investigate and report upon dangers incident to electric lighting, if any, and the means of overcoming them, and the other to examine and report upon the principles which should govern the erection of fire escapes and lifts or elevators in new buildings and in those now erected."

Mr. McKean said that he particularly desired to have the committee on fire escapes appointed as, although the law required the erection of such escapes, there was at present no guide as to what constituted a safe escape. He was satisfied in his own mind that iron ladders were unsuited to the use of women and children, and

thought it important to have the whole subject carefully considered by builders and practical men, so that property owners might be given a reliable guide as to what they ought to erect.

Mr. Robert Grimshaw seconded the resolution, and suggested that elevator shafts be also considered by the committee on fire escapes, which suggestion was accepted by Mr. McKean and his resolution amended in accordance therewith. Mr. Grimshaw said that the iron ladders would be very likely in winter to prove practically useless. At the best of times, only workmen used to descending ladders could escape by them; but when cold enough to take the skin of the palm out of one's hands, or when covered with ice, most people would prefer to jump from the windows rather than run the risk of descending by them.

The resolution was unanimously adopted, and the President afterwards appointed the following committees:

On Electric Lighting.—Dr. R. E. Rogers, Dr. C. M. Cresson, David Brooks, Alex. E. Outerbridge, Jr., W. W. Griscom, E. Alex. Scott, Prof. E. J. Houston.

On Fire Escapes and Elevators.—Wm. B. Bement, John Baird, R. K. Betts, Frederick Graff, C. H. Banes, Strickland Kneass, Prof. Wm. D. Marks, Henry G. Morris, J. B. Lippincott.

The following memoir of Henry Cartwright, the late Vice President, prepared by Mr. Washington Jones, was read by the Secretary:

"It is with sorrow that I announce to you officially, the death of our late fellow-member and Vice President, Henry Cartwright, which occurred through an accident early in July last. Mr. Cartwright was for many years an active and influential member of the Franklin Institute, one of its managers and, since January, 1880, one of its Vice Presidents.

"In the discharge of the duties imposed by the position to which you had chosen him he was prompt and efficient, and he willingly gave his time and abilities to promote the interests of the Institute. In the deliberations of the Committee on Science and the Arts, of which he was a valued member of long standing, he always bore part, and his judicial mind and his matured judgment, especially upon mechanical subjects, aided in the formation of just conclusions. As a presiding officer, he maintained the dignity of the chair by calm and courteous demeanor and impartiality in ruling. Commencing his career as a machinist, he soon enlarged his sphere of use-